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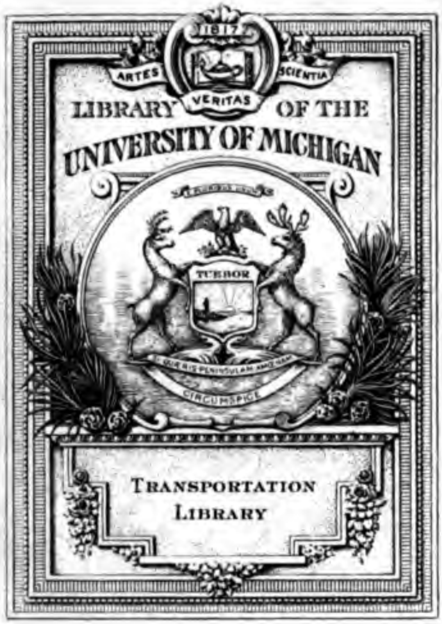

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MECHANICAL PHILOSOPHY.

By JOHN ROBISON, LL. D.
LATE PROFESSOR OF NATURAL PHILOSOPHY IN THE
UNIVERSITY OF EDINBURGH.

WITH NOTES,

By DAVID BREWSTER, LL. D.
FELLOW OF THE ROYAL SOCIETY OF LONDON, AND SECRETARY TO THE
ROYAL SOCIETY OF EDINBURGH.

IN FOUR VOLUMES,
AND A VOLUME OF PLATES.

VOL. II.

EDINBURGH:
PRINTED FOR JOHN MURRAY, LONDON.

1822.

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THE ARTICLES STEAM AND STEAM-ENGINES

WRITTEN FOR THE ENCYCLOPÆDIA BRITANNICA,

BY

JOHN ROBISON, LL.D. F.R.S.E. &c.

LATE PROFESSOR OF NATURAL PHILOSOPHY IN THE UNIVERSITY
OF EDINBURGH, &c.

WITH NOTES AND ADDITIONS,

BY

JAMES WATT, LL.D. F.R.S. L. & E.

MEMBER OF THE NATIONAL INSTITUTE OF FRANCE, AND OF THE
BATAVIAN SOCIETY OF ROTTERDAM.

AND

A LETTER ON SOME PROPERTIES OF STEAM

BY THE LATE JOHN SOUTHERN, Esq.

LETTER

TO

Dr BREWSTER from Mr WATT.

Dear Sir,

At your request, I have carefully perused my late excellent friend Dr Robison's articles "Steam" and "Steam-Engines," in the Encyclopaedia Britannica, and have made remarks upon them in such places where, either from the want of proper information, or from too great a reliance on the powers of his extraordinary memory, at a period when it probably had been weakened by a long state of acute pain, and by the remedies to which he was obliged to have recourse, he had been led into mistakes in regard to facts, and also in some places where his deductions have appeared to me to be erroneous.

There had been but very little interchange of letters between us for some years previous to his writing those articles, and our opportunities of meeting had been rare, and of short duration, and not occupied by philosophical discussions. Had I been apprised of his design, I might at least have prevented the errors respecting the facts in which I was concerned; but, upon the whole, it is more surprising to me that his recollection should have served him so well in narrating transactions of 30 years standing, than that it should sometimes have led him astray. If I had not retained some memorandums made at the time of, or soon after, their occurrence, I should myself have felt great difficulty in recalling to mind the particulars at the period when I first perused those articles, which was some time after their publication. I had about that period an opportunity of personally stating to Dr Robison some remarks upon them, of which he availed himself to a small extent in the Supplement to the Encyclopædia Britannica, and probably would have done so still more, had he been called upon to remould these articles.

I have endeavoured to throw most of my corrections into the form of notes; but in some places I judged it necessary to alter the text; which alterations I have marked to be printed in Italics, that they may be readily distinguished from the original. In a few places I have cancelled part of the text without any substitution, none appearing to me to be required. In others I have left part of the reasoning unaltered which I did not concur in; as in mere matters of opinion, where no manifest error was involved, I did not conceive it proper to introduce my own speculations.

As the subjects of Steam, and Steam-Engines, had been almost dismissed from my mind for many years previous to my undertaking this revision, I have called in the aid of my friend Mr John Southern, and of my son, whose daily avocations in the manufacture of steam-engines, render them more conversant with some points, to direct my attention to them; and of the former, to examine such of the algebraic formulæ as appeared essential, an office for which he is much better qualified than myself; and he has accordingly marked those formulæ with his initials.

I have not attempted to render Dr Robison's memoir a complete history of the Steam-Engine; nor have I even given a *detailed* account of my own improvements upon it. The former would have been an undertaking beyond my present powers, and the latter must much have exceeded the limits of a commentary upon my friend's work. I have therefore confined myself to correcting such parts as appeared necessary, and to adding such matter as he had not an opportunity of knowing.

Here it was my intention to have closed this letter; but the representations of friends, whose opinions I highly value, induce me to avail myself of this opportunity of noticing an error into which not only Dr Robison, but apparently also Dr Black, has fallen, in relation to *the origin* of my improvements upon the Steam-Engine, and which not having been publicly controverted by me, has, I am informed, been adopted by almost every subsequent writer upon the subject of Latent Heat.

Dr Robison, in the article Steam-Engine, after passing an encomium upon me, dictated by the partiality of friendship, qualifies me as the "*pupil* and intimate friend of Dr Black;" a description which, not being there accompanied with any inference, did not particularly strike me at the time of its first perusal. He afterwards, in the dedication to me of his edition of Dr Black's Lectures upon Chemistry, goes the length of supposing me to have professed to owe my improvements upon the Steam-Engine to the instructions and information I had received from that gentleman, which certainly was a misapprehension, as, though I have always felt and acknowledged my obligations to him for the information I had received from his conversation, and particularly for the knowledge of the doctrine of Latent Heat, I never did, nor *could*, consider my improvements as originating in those communications. He is also mistaken in his assertion, p. 8 of the Preface to the above work, that " I had attended two " courses of the Doctor's Lectures ;"* for, unfortunately for me, the necessary avocations of my business prevented me from attending his or any other lectures at College; and as Dr ROBISON was himself absent from Scotland for four years at the period referred to, he must have been misled by erroneous information. In page 184 of the Lectures, Dr BLACK says, " I have the pleasure of thinking that the knowledge we have acquired concerning " the nature of elastic vapours, in consequence of my fortunate observation of what happens in its formation and " condensation, has contributed in no inconsiderable degree to the public good, by *suggesting* to my friend Mr " WATT of Birmingham, then of Glasgow, his improvement on this useful engine," (meaning the steam-engine, of which he is then speaking). There can be no doubt, from what follows in his description of the engine, and from the very honourable mention which he has made of me in various parts of his Lectures, that he did not mean to lessen any merit that might attach to me as an inventor; but, on the contrary, he always was disposed to give me fully as much praise as I deserved. And were that otherwise doubtful, it would, I think, be evident from the following quotation from a letter of his to me, dated 13th February, 1783, where, speaking of an intended publication by a friend of mine on subjects connected with the history of steam, he says, " I think it is very proper for " you to give him a short account of your discoveries and " speculations, and *particularly to assert clearly and fully*

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* Repeated more in detail, with the same erroneous inferences, in his note, vol. I. p. 504.

"*your sole right to the honour of the improvements of the* "*Steam-Engine;*" and in a written testimonial which he very kindly gave on the occasion of a trial at law against a piracy of my invention in 1796–7, after giving a short account of the invention, he adds, "*Mr Watt was the sole* "*inventor of the capital improvement and contrivance above* "*mentioned.*"

Under this conviction of his candour and friendship, it is very painful to me to controvert any assertion or opinion of my revered friend; yet in the present case I find it necessary to say, that he appears to me to have fallen into an error, and I hope, in addition to my assertion, to make that appear by the short history I have given of my invention in my notes upon Dr ROBISON's essay, as well as by the following account of the state of my knowledge previous to my receiving any explanation of the doctrine of Latent Heat, and also from that of the facts which principally guided me in the invention.

It was known very long before my time, that steam was condensed by coming into contact with cold bodies, and that it communicated heat to them. Witness the common still, &c. &c.

It was known by some experiments of Dr CULLEN, and others, that water and other liquids boiled in vacuo at very low heats; water below 100°.

It was known to some philosophers, that the capacity or equilibrium of heat, as we then called it, was much smaller in mercury and tin than in water.

It was also known, that evaporation caused the cooling of the evaporating liquid, and bodies in contact with it.

I had myself made experiments to determine the following facts.

1st, The capacities for heat of iron, copper, and some sorts of wood, comparatively with water. Similar experiments had also subsequently been made by Dr IRVINE, on these and other metals.

2d, The bulk of steam was compared with that of water.

3d, The quantity of water which could be evaporated in a certain boiler by a pound of coals.

4th, The elasticities of steam at various temperatures greater than that of boiling water, and an approximation to the law which it followed at other temperatures.

5th, How much water, in the form of steam, was required every stroke by a small NEWCOMEN's engine, with a wooden cylinder six inches diameter, and twelve inches long in the stroke.

6th, I had measured the quantity of cold water required in every stroke to condense the steam in that cylinder, so as to give it a working power of about 7 lb. on the inch.

Here I was at a loss to understand how so much cold water could be heated so much by so small a quantity in the form of steam, and applied to Dr BLACK, as is related in the short history, p. 116, Note, and then first understood what was called Latent Heat.

But this theory, though useful in determining the quantity of injection necessary where the quantity of water evaporated by the boiler, and used by the cylinder, was known, and in determining, by the quantity and heat of the hot water emitted by NEWCOMEN's engines, the quantity of steam required to work them, did not lead to the improvements I afterwards made in the engine. These improvements proceeded upon the old-established fact, that steam was condensed by the contact of cold bodies, and the later known one, that water boiled in vacuo at heats below 100°, and consequently that a vacuum could not be obtained unless the cylinder and its contents were cooled every stroke to below that heat.

These, and the degree of knowledge I possessed of the elasticities of steam at various heats, were the principal things it was *necessary* for me to consider in contriving the new engine. They pointed out that, to avoid useless condensation, the vessel in which the steam acted upon the piston ought always to be as hot as the steam itself; that to obtain a proper degree of exhaustion, the steam must be condensed in a *separate vessel*, which might be cooled to as low a degree as was necessary, without affecting the cylinder; and that as the air and condensed water could not be blown *out* by the steam as in NEWCOMEN'S, they must be extracted by a pump, or some other contrivance; that, in order to prevent the necessity of using water to keep the piston air-tight, and also to prevent the air from cooling the cylinder during the descent of the piston, it was necessary to employ steam to act upon the piston in place of the atmosphere. Lastly, to prevent the cylinder from being cooled by the external air, it was proper to inclose it in a case containing steam, and again to inclose that in a case of wood, or of some other substance which transmitted heat slowly.

Although Dr Black's theory of latent heat did not *suggest* my improvements on the steam-engine, yet the knowledge upon various subjects which he was pleased to communicate to me, and the correct modes of reasoning, and of making experiments of which he set me the example, certainly conduced very much to facilitate the progress of my inventions; and I still remember with respect and gratitude the notice he was pleased to take of me when I very little merited it, and which continued throughout his life.

To Dr ROBISON I am also bound to acknowledge my obligations for very much information and occasional assistance in my pursuits, and above all, for his friendship, which ended only with his life; a friendship which induced him, when I was beset with an host of foes, to come to London in the depth of winter, and appear as a witness for me in a court of justice, whilst labouring under an excessively painful disorder, which ultimately deprived him of life. To the remembrance of that friendship is principally owing my taking upon myself the office of his commentator at my advanced age.

May I request, sir, that you and the public will permit that age to be my excuse for any errors I may have committed, and for any deficiencies in the performance of an office which at no period would have been congenial to my habits; and allow me to remain, with esteem,

Dear Sir,

Your most obedient humble servant,

JAMES WATT.

Heathfield, May 1814.

ON STEAM.

1. STEAM, is the name given in our language to the visible moist vapour which arises from all bodies which contain juices easily expelled from them by heats not sufficient for their combustion. Thus we say, the steam of boiling water, of malt, of a tan-bed, &c. It is distinguished from smoke by its not having been produced by combustion, by not containing any soot, and by its being condensible by cold into water, oil, inflammable spirits, or liquids composed of these.

2. We see it rise in great abundance from bodies when they are heated, forming a white cloud, which diffuses itself and disappears at no very great distance from the body from which it was produced. In this case the surrounding air is found loaded with the water or moisture which seems to have produced it, and the steam seems to be completely soluble in air, as salt is in water, composing, while thus united, a transparent elastic fluid.

3. But in order to its appearance in the form of an opaque white cloud, the mixture with or dissemination in air, *or in some elastic fluid colder than itself*, seems absolutely necessary. If a tea-kettle boils violently, so that

the steam is formed at the spout in great abundance, it may be observed, that the visible cloud is not formed at the very mouth of the spout, but at a small distance before it, and that the vapour is perfectly transparent at its first emission. This is rendered still more evident by fitting to the spout of the tea-kettle a glass pipe of any length, and of as large a diameter as we please. The steam is produced as copiously as without this pipe, but the vapour is transparent through the whole length of the pipe. Nay, if this pipe communicate with a glass vessel terminating in another pipe, and if the vessel be kept sufficiently hot, the steam will be as abundantly produced at the mouth of this second pipe as before, and the vessel will be quite transparent. The visibility therefore of the matter which constitutes the steam, is an accidental or extraneous circumstance, and requires the admixture with air; yet this quality again leaves it when united with air by solution. It appears therefore to require a *dissemination* in the air. The appearances are quite agreeable to this notion: for we know that one perfectly transparent body, when minutely divided and diffused among the parts of another transparent body, but not dissolved in it, makes a mass which is visible. Thus oil beaten up with water makes a white opaque mass.

4. In the mean time, as steam is produced, the water gradually wastes in the tea-kettle, and will soon be totally expended, if we continue it on the fire. It is reasonable therefore to suppose, that this steam is nothing but water changed by heat into an aerial or elastic form. If so, we should expect that the privation of this heat would leave it in the form of water again. Accordingly this is fully verified by experiment; for if the pipe fitted to the spout of the tea-kettle be surrounded with cold water, no steam will issue, but water will continually trickle from it in drops: and if the process be conducted with the proper precautions, the water which we thus obtain from the

pipe will be found equal in quantity to that which disappears from the tea-kettle.

5. This is evidently the common process for distilling; and the whole appearances may be explained by saying, that the water is converted by heat into an elastic vapour, and that this, meeting with colder air, imparts to it the heat which it carried off as it arose from the heated water, and being deprived of its heat, it is again water. The particles of this water being vastly more remote from each other than when they were in the tea-kettle, and thus being disseminated in the air, become visible, by reflecting light from their anterior and posterior surfaces, in the same manner as a transparent salt becomes visible when reduced to a fine powder. This disseminated water being presented to the air in a very extended surface, is quickly dissolved by it, as pounded salt is in water, and again becomes a transparent fluid, but of a different nature from what it was before, being no longer convertible into water by depriving it of its heat.

6. Accordingly this opinion, or something very like it, has been long entertained. Muschenbroeck expressly says, that the water in the form of vapour carries off with it all the heat which is continually thrown in by the fuel. But Dr Black was the first who attended minutely to the whole phenomena, and enabled us to form distinct notions of the subject. He had discovered, that it was not sufficient for converting ice into water that it be raised to that temperature in which it can no longer remain in the form of ice. A piece of ice of the temperature 32° of Fahrenheit's thermometer, will remain a very long while in air of the temperature 50° before it be all melted, remaining all the while of the temperature 32°, and therefore continually absorbing heat from the surrounding air. By comparing the time in which the ice had its temperature changed from 28° to 32°, with the subsequent time of its complete lique-

faction, he found that it absorbed about 130 or 140 times as much heat as would raise its temperature one degree; and he found that one pound of ice, when mixed with one pound of water 140 degrees warmer, was just melted, but without rising in its temperature above 32°. Hence he justly concluded, that water differed from ice of the same temperature by containing, as a constituent ingredient, a great quantity of fire, or of the cause of heat, united with it in such a way as not to quit it for another colder body, and therefore so as not to go into the liquor of the thermometer and expand it. Considered therefore as the possible cause of heat, it was latent, which Dr Black expressed by the abbreviated term, LATENT HEAT. If any more heat was added to the water it was not latent, but would readily quit it for the thermometer, and by expanding the thermometer, would show what is the degree of this *redundant* heat, while fluidity alone is the indication of the *combined* and latent heat.

Dr Black, in like manner, concluded, that in order to convert water into an elastic vapour, it was necessary, not only to increase its uncombined heat till its temperature is 212°, in which state it is just ready to become elastic, but also to pour into it a great quantity of fire, or the cause of heat, which combines with every particle of it, so as to make it repel, or to recede from, its adjoining particles, and thus to make it a particle of an elastic fluid.* He supposed that this additional heat might be combined with it so as not to quit it for the thermometer; and therefore so as to be in a latent state, having elastic fluidity for its sole indication.

7. This opinion was very consistent with the phenomenon of boiling off a quantity of water. The application of

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* See Dr Black's own account of this matter in his Lectures, published by Dr Robison.

heat to it causes it gradually to rise in its temperature till it reaches the temperature 212°. It then begins to send off elastic vapour, and is slowly expended in this way, continuing all the while of the same temperature. The steam also is of no higher temperature, as appears by holding a thermometer in it. We must conclude that this steam contains all the heat which is expended in its formation. Accordingly the scalding power of steam is well known; but it is extremely difficult to obtain precise measures of the quantity of heat absorbed by water during its conversion into steam. Dr Black endeavoured to ascertain this point, by comparing the time of raising its temperature a certain number of degrees with the time of boiling it off by the same external heat; and he found that the heat latent in steam, which balanced the pressure of the atmosphere, was not less than 800 degrees. He also directed Dr Irvine of Glasgow to the form of an experiment for measuring the heat actually extricated from such steam during its condensation in the refrigeratory of a still, which was found to be not less than 774 degrees. Dr Black was afterwards informed by Mr Watt, that a course of experiments, which he had made in each of these ways with great precision, in 1781,[*] determined the latent

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[*] "The following is the account of my experiments on latent heats, made in February and March 1781.

"A pipe of copper 5-8ths of an inch diameter inside, 1-50th of an inch thick, and 5 feet long, having 3 inches of one of its ends bent downwards, was fixed steam-tight on the spout of a tea-kettle, from which the pipe inclined upwards, so that the bent end was about two feet higher than the spout of the kettle; and a cork perforated with a hole of about 2-10ths of an inch diameter, kept open by a bit of quill, was fixed in the opening of the bent end.

"The tea-kettle was filled with water half way up the entry of the spout; the lid was fixed on tight with some oatmeal dough, and held down by a piece of wood reaching up to the handle. A tin pan, 4 inches deep and 6 inches diameter, had 2½ pounds avoirdupois of water put into it, which heat of steam under the ordinary pressure of the atmosphere to be about 948 or 950 degrees. Mr Watt also found that water would distil *in vacuo* when of the tem-

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filled it nearly to 2½ inches deep. The water was weighed very accurately; the tin pan and a disk of strong paper (oiled with linseed oil and dried in a stove,) fitted to its inside, being first counterpoised when they were quite dry.

"The pan and water were placed upon several folds of flannel on a stand, and the extremity of the cork in the bent end of the pipe was immersed in the water. The water in the kettle was made to boil for some time before the end of the pipe was immersed in the cold water in the pan, which was not done until it was observed that no water dropped from it, but that all the condensed part of the steam returned by the inclined tube to the tea-kettle.

"When the end of the tube was immersed in the cold water in the pan, the steam issuing from it was condensed with a crackling noise, and began to heat the water in contact with it. The water being constantly stirred with a circular motion, the heat was thereby diffused equably throughout the whole, and the experiment was continued until the water had acquired the heat of from 70° to 90°, which happened generally in from 4 to 6 minutes. Immediately after the thermometer had shewn what the heat was, which was in less than half a minute, the water was covered by the disk of oiled paper, to prevent evaporation; which would otherwise have lessened its weight during the operation of weighing. The thermometer employed became stationary in about 10 seconds.

"When the experiments were finished, the tin pan, made quite dry, was set in a room where the air was about 40°, and stood there for half an hour, when it was thought to have acquired the heat of the place, two pounds of water at 76° were then poured into it, and the heat was found to be 75½°.

"Then for every 35½° with two pounds of water, or for every 44° with 2½ pound of water, half a degree must be allowed for the heat absorbed by the pan.

"The heat of the room, when the experiments were made, was generally about 56°.

"Eleven experiments were made in the foregoing manner, from which the latent heat was calculated according to the following example.

EXPERIMENT I. FEBRUARY 23, 1781.

"The heat of the water in the pan, on beginning the experiment, was 43.5°. When the experiment was ended, the heat of that water was 89.5, consequently it had gained 46° from the steam it had condensed. The perature 70°; and that in this case *the latent heat of the steam appeared to be about* 1000°, *and some other experiments* made him suppose that the sum of the sensible and latent

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weight of the water on commencing the experiment, was 2½ pounds avoirdupois, or 17,500 grains; after the experiment, its weight was 18,260 grains, consequently it had gained 760 grains from the condensed steam. Thus, multiplying 17,500 grains by 46°, the heat received from the condensed steam, and 0.5° the heat absorbed by the pan, = 46°.5, we have 8,137,500°. which, divided by 760 grains, the weight of the water which in the state of steam communicated the heat, we have 1070°; to which we must add the heat retained, being that of the mixture, = 89°.5, which produces 1159°.5, as the sum of the sensible and latent heat of the steam; and deducting 212°, the sensible heat, we have the latent heat 947.5.

"The results and other particulars of the eleven experiments, tried and calculated in the same manner, are shewn in the following Table.

No. of Experiments.	Quantity of cold Water in Pan.	Temperature of the cold Water.	Weight of the condensed Steam.	Temperature of the heated Water.	Increase of Heat.	Total sensible and latent Heat.	Latent Heat.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.
	Grains.	°	Grains.	°	°	°	
1.	17500	43.5	760.	89.5	46.5	1159.5	947.5
2.	17500	44.5	708.	86.5	42.5	1136.9	924.9
3.	17500	44.5	899.	98.	54.	1149.1	937.1
4.	17500	44.5	467.5	73.5	29.5	1175.6	963.6
5.	17500	44.5	369.	67.25	23.	1152.	940.
6.	17500	47.5	642.	87.	40.	1177.3	965.3
7.	17500	49.	588.5	84.5	36.	1155.	943.
8.	17500	47.	675.	87.5	41.	1150.5	938.5
9.	17500	45.	680.5	86.5	42.	1166.5	954.7
10.	17500	45.	664.25	85.5	41.	1165.66	953.66
11.	17500	45.	975.	102.	57.5	1134.	922.

"Adding together the sums in the 8th column, and dividing by 11, gives 945°.3 for the average, but the 2d and 11th experiments, giving a much smaller result than any of the others, appear to me to be objectionable, and if rejected, will give 949°.9 for the average.

heats is a constant quantity. This is a curious and not an improbable circumstance; but we have no information of

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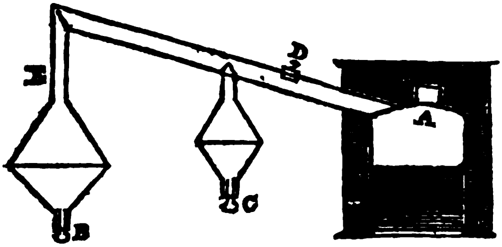
" But there being several causes which affect the results of these experiments, and which for the most part tend to give the latent heat rather less than what it probably is, we shall not err in excess by calling the latent heat 960°. W.*

" Not being satisfied with the experiments I had tried upon the latent heat of distillation in vacuo at Glasgow in 1765, which were made in a hasty manner, I made other experiments in 1783, the result of which still did not prove satisfactory. I shall, however, give a short account of the apparatus, and of the only experiments of which I have retained minutes, hoping it may excite some person to ascertain the matter more accurately.



" A small still A, surrounded by a balneum, was made of tin plate, in the form annexed, which communicated by a pipe with the two double cones B and C, each of which had a very small opening in its lower apex, shut air-tight by a brass plug. There was also an opening, shut in the same manner, in the tube at D. The conical mouth of the still at A was shut by a good cork.

" A pint of water was poured into the inside vessel, and as much into the outside one. The whole was then set upon a chafing dish and made to boil. The steam was allowed to issue at B and C until it was supposed that all the air was extruded. The aperture C was then shut, and just immersed in a vessel of water, to prevent the air from entering. The steam was allowed to issue some time longer at B, and it was also shut, and immediately immersed to a small depth in water. Cold water was then poured into the balneum, so as to cover the orifice and its cork. A degree of exhaustion was instantly produced in the internal vessel, and in the two double cones communicating with it. The double cone B was then the particulars of these experiments. The conclusion evidently presupposes a knowledge of that particular temper-

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* " In 1803 Mr Southern examined with great care the latent heat of steam at the temperatures of 229°, 270°, and 295°, and after making the due corrections, found it to be 942°, 942°, and 950°; for the particulars of which, I refer to his own account of those experiments, in a letter to me, which I have placed at the end of my remarks on Dr Robison's articles."

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wholly immersed in a tin pan 6 inches deep, and 8¾ inches diameter, filled with cold water to within an inch of its mouth. This water weighed 130 oz. 6 dr. 40 gr. troy weight, or 62800 grains. Its heat at the beginning of the experiment was 52° (say 51°75). When it was supposed a sufficient quantity had distilled into the receiver B, the heat of the water in the refrigeratory was 61°, consequently had increased 9° + (say 9¼°). The plug at D was withdrawn, and the air admitted. The refrigeratory was removed, and the double cone B being wiped dry, its plug was withdrawn, and the water it contained let out, its heat examined, and then weighed. The heat was 62°, and its weight was 1 oz. and 54 gr., or 534 gr., to which 6 gr. were added by estimation, for water adhering to the inside of the cone, in all 540 gr. The heat of the water in the balneum at the beginning of the experiment was 134°, and at the end 158°; consequently at the latter period about one-third part of air, or other elastic vapour, remained in the still and receiver. The duration of the experiment was 9 minutes. The heat of the chafing dish was prevented from affecting the refrigeratory by a screen of bricks. The heat of the air in the room was about 58°.

"The double cone B weighed 1000 grains, and as it was of 134° of heat at the beginning, and was cooled to 62° by the refrigeratory, it lost 72°. Its specific gravity was probably about 7½ times that of water, and consequently its bulk that of $\frac{100}{7.5}$ = 134.6 grains of water, and its capacity for heat being about ¾ of that of the same bulk of water, it would contain the same quantity of heat as about 101 grains of water; and this heat not being communicated by the condensed steam, contained in the cone at the end of the operation, is to be deducted from the heat acquired by the water in the refrigeratory, or, which is the same thing, 101 grains are to be deducted from its weight.

"The result of the experiment may be stated as follows:

"The weight of the water in the refrigeratory	62800 grains
Deduct 101 grains as the equivalent for the bulk of the cone	101
Remainder	62699
Add the heat absorbed by the refrigeratory which was of tin plate, and weighed 24½ oz.; but for the wire round its mouth, and other parts nót in contact with the water, I allow 4½ oz.: remains 20 oz. = in bulk to about 1320 grains of water, but its capacity for heat being only ¾ of that of water, is equal only to 980 grains of water, which is to be added to the water in the refrigeratory.	980

ature in which the water has no heat; but this is a point which is still *sub judice*.*

8. This conversion of liquids (for it is not confined to water, but obtains also in ardent spirits, oils, mercury,

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"Total weight of the water, &c. heated	63679 grains
which multiplied by 9°.25 the heat acquired =	589030.75
and divided by the weight of the condensed steam = 540 grains gives	1090°.79
to which, adding the heat retained =	62°.
gives the sum of the sensible and latent heat =	1152.79
from which, deducting the average sensible heat of the steam	146.
gives the latent heat at that temperature =	1006.79

"I have said that I am by no means satisfied with the accuracy of this experiment. Too many things are taken by estimation which ought to have been ascertained by experiment, and the degree of exhaustion was not so great as it might have been, or as I have obtained in some other experiments. The cone B ought to have had a slip joint in its neck at E, by which it could have been taken off and weighed before and after each experiment, to ascertain the quantity of water which adhered to its inside.

"Its specific gravity ought also to have been determined by weighing it in water, and its capacity for heat examined accurately. The cone C was intended to catch any water which might come over in consequence of a violent ebullition; but by cooling the still sooner than the cones, that did not take place, and the cone C was superfluous, and might be omitted in another case.

"The vacuum ought to have been made more perfect by repeatedly boiling the water, condensing the steam, and blowing out the air, until the distillation should take place at 70° or 80°. All this I should probably have done, had I not been called away by business from prosecuting the experiment at that time, and my attention having never been drawn to it since. Now it is too late." W.

* "In my first experiments, made in a rude way in 1765, the latent heat appeared to be above 1200°, but later experiments, which I have related in the preceding note, have made me suppose the former ones to be erroneous.

"Mr Southern is inclined to conclude, from the experiments on the latent heat of steam at high temperatures, which I have mentioned, that the *latent heat* is a constant quantity, instead of the sum of the latent and sensible heats being so. Dr Priestley favoured me with his company during the ninth and tenth experiments in 1781 and in 1783; Mr De Luc attended a repetition of these experiments, and the distillations in vacuo." W.

&c. is the cause of their boiling. The heat is applied to the bottom and sides of the vessel, and gradually accumulates in the fluid, in a sensible state, uncombined, and ready to quit it and to enter into any body that is colder, and to diffuse itself between them. Thus it enters into the fluid of a thermometer, expands it, and thus gives us the indication of the degree in which it has been accumulated in the water; for the thermometer swells as long as it continues to absorb sensible heat from the water: and when the sensible heat in both is in equilibrio, in a proportion depending on the nature of the two fluids, the thermometer rises no more, because it absorbs no more heat or fire from the water; for the particles of water which are in immediate contact with the bottom, are now (by this gradual expansion of liquidity) at such distance from each other, that their laws of attraction for each other and for heat are totally changed. Each particle either no longer attracts, or perhaps it repels its adjoining particle, and now accumulates round itself a great number of the particles of heat, and forms a particle of elastic fluid, so related to the adjoining new-formed particles, as to repel them to a distance *about twelve and a half* times greater than their distance in the state of water. Thus a mass of elastic vapour of sensible magnitude is formed. Being *about two thousand* times lighter than an equal bulk of water, it must rise up through it, as a cork would do, in a form of transparent ball or bubble, and getting to the top, it dissipates, filling the upper part of the vessel with vapour or steam.

9. Thus, by tossing the liquid into bubbles, which are produced all over the bottom and sides of the vessel, it produces the phenomenon of ebullition or boiling. Observe, that during its passage up through the water, it is not changed or condensed; for the surrounding water is already so hot that the sensible or uncombined heat in it, is in equilibrio with that in the vapour, and therefore it is not disposed to absorb any of that heat which is combined

as an ingredient of this vapour, and gives it its elasticity. For this reason, it happens that water will not boil till its whole mass be heated up to 212°; for if the upper part be colder, it robs the rising bubble of that heat which is necessary for its elasticity, so that it immediately collapses again, and the surface of the water remains still. This may be perceived by holding water in a Florence flask over a lamp or choffer. It will be observed, some time before the real ebullition, that some bubbles are formed at the bottom, and get up a very little way, and then disappear. The distances which they reach before collapsing increase as the water continues to warm farther up the mass, till at last it breaks out into boiling. If the handle of a tea-kettle be grasped with the hand, a tremor will be felt for some little time before boiling, arising from the little succussions which are produced by the collapsing of the bubbles of vapour. A much more violent, and really a remarkable phenomenon appears, if we suddenly plunge a lump of red-hot iron into a vessel of cold water, taking care that no red part be near the surface. If the hand be now applied to the side of the vessel, a most violent tremor is felt, and sometimes strong thumps: these arise from the collapsing of very large bubbles. If the upper part of the iron be too hot, it warms the surrounding water so much, that the bubbles from below come up through it uncondensed, and produce ebullition without this succussion. The great resemblance of this tremor to the feeling which we have during the shock of an earthquake, has led many to suppose that these last are produced in the same way; and their hypothesis, notwithstanding the objections which we have elsewhere stated to it, is by no means unfeasible.

10. It is owing to a similar cause that violent thumps are sometimes felt on the bottom of a tea-kettle, especially one which has been long in use. Such are frequently crusted on the bottom with a stony concretion. This

sometimes is detached in little scales. When one of these is adhering by one end to the bottom, the water gets between them in a thin film. Hence it may be heated considerably above the boiling temperature, and it suddenly rises up in a large bubble, which collapses immediately. A smooth shilling lying on the bottom will produce this appearance very violently, or a thimble with the mouth down.

11. In order to make water boil, the fire must be applied to the bottom or sides of the vessel. If the heat be applied at the top of the water, it will waste away without boiling; for the very superficial particles are first supplied with the heat necessary for rendering them elastic, and they fly off without agitating the rest. *

12. Since this disengagement of vapour is the effect of its elasticity, and since this elasticity is a determined force when the temperature is given, it follows, that fluids cannot

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* "We explained the opaque and cloudy appearance of steam, by saying that the vapour is condensed by coming into contact with the cooler air. There is something in the form of this cloud which is very inexplicable. The particles of it are sometimes very distinguishable by the eye; but they have not the smart star-like brilliancy of very small drops of water, but give the fainter reflection of a very thin film or vesicle like a soap bubble. If we attend also to their motion, we see them descending very slowly in comparison with the descent of a solid drop; and this vesicular constitution is established beyond a doubt by looking at a candle through a cloud of steam. It is seen surrounded by a faint halo with prismatical colours, precisely such as we can demonstrate by optical laws to belong to a collection of vesicles, but totally different from the halo which would be produced by a collection of solid drops. It is very difficult to conceive how these vesicles can be formed of watry particles, each of which was surrounded with many particles of fire, now communicated to the air, and how each of these vesicles shall include within it a ball of air; but we cannot refuse the fact. We know, that if, while linseed oil is boiling, or nearly boiling, the surface be obliquely struck with the ladle, it will be dashed into a prodigious number of exceedingly small vesicles, which will float about in the air for a long while. Mr Saussure was (we think) the first who distinctly observed this vesicular form of mists and clouds; and he makes considerable use of it in explaining several phenomena of the atmosphere." Dr R.

boil till the elasticity of the vapour overcomes the pressure of the incumbent fluid and of the atmosphere. Therefore, when this pressure is removed or diminished, the fluids must sooner overcome what remains, and boil at a lower temperature. Accordingly it is observed that water will boil in an exhausted receiver when of the heat of the human body. If two glass balls, A and B, (Plate 1. Fig. 1.) be connected by a slender tube, and one of them A be filled with water (a small opening or pipe *b* being left at top of the other), and this be made to boil, the vapour produced from it will drive all the air out of the other, and will at last come out itself, producing steam at the mouth of the pipe. When the ball B is observed to be occupied by transparent vapour, we may conclude that the air is completely expelled. Now shut the pipe by sticking it into a piece of tallow or bees-wax, the vapour in B will soon condense, and there will be a vacuum. The flame of a lamp and blow-pipe being directed to the little pipe, will cause it immediately to close and seal hermetically. We now have a pretty instrument or toy called a PULSE-GLASS.* Grasp the ball A in the hollow of the hand; the heat of the hand will immediately expand the bubble of vapour which may be in it, and this vapour will drive the water into B, and then will blow up through it for a long while, keeping it in a state of violent ebullition, as long as there remains a drop or film of water in A. But care must be taken that B is all the while kept cold, that it may condense the vapour as fast as it rises through the water. Touching B with the hand, or breathing warm on it, will immediately stop the ebullition in it. When the water in A has thus been dissipated, grasp B in the hand; the water will be driven into A, and the ebullition will take place there as it did in B. Putting one of the balls into the mouth will make the ebullition more violent in the other, and the one in the mouth will feel very cold. This is a pretty illustration of the rapid absorption of the heat by the particles of water which are thus converted into elastic vapour. We have seen this little toy suspended by the *middle* of the tube like a balance, and thus placed in the *inside* of a window, having two holes *a* and *b* cut in the pane, in such a situation that when A is full of water and preponderates, B is opposite to the hole *b*. Whenever the room became sufficiently warm, the vapour was formed in A, and immediately drove the water into B, which was kept cool by the air coming into the room through the hole *b*. By this means B was made to preponderate in its turn, and A was then opposite to the hole *a*, and the process was now repeated in the opposite direction; and this amusement continued as long as the room was warm enough.

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* "The invention of the pulse glass is ascribed to Dr Franklin, its date uncertain, probably subsequent to my improvement of the Steam Engine, at least certainly not known to me at that time.

"The boiling in vacuo was known long before the pulse-glass was invented. Pulse-glasses succeed bettor when filled with spirits of wine than with water." W.

13. We know that liquors differ exceedingly in the temperatures necessary for their ebullition. This forms the great chemical distinction between volatile and fixed bodies. But the difference of temperature in which they boil, or are converted into permanently elastic vapour, under the pressure of the atmosphere, is not a certain measure of their differences of volatility. The natural boiling point of a body is that in which it will be converted into elastic vapour under no pressure, or *in vacuo*. The boiling point in the open air depends on the law of the elasticity of the vapour in relation to its heat. A fluid A may be less volatile, that is, may require more heat to make it boil *in vacuo*, than a fluid B: But if the elasticity of the vapour of A be more increased by an increase of temperature than that of the vapour of B, A may boil at as low, or even at a lower temperature, in the open air, than B does; for the increased elasticity of the vapour of A may sooner over-

come the pressure of the atmosphere.* Few experiments have been made on the relation between the temperature and the elasticity of different vapours. So long ago as the year 1765, we had occasion to examine the boiling points of all such liquors as we could manage in an air-pump; that is, such as did not produce vapours which destroyed the valves and the leathers of the pistons: and we thought that the experiments gave us reason to conclude, that the elasticity of all the vapours was affected by heat nearly in the same degree.†

14. For we found that the difference between their boiling points in the air and *in vacuo* was nearly the same in all, namely, about 120 degrees of Fahrenheit's thermometer. It is exceedingly difficult to make experiments of this kind: The vapours are so condensible, and change their elasticity so prodigiously by a trifling change of temperature, that it is almost impossible to examine this point with precision. It is, however, as we shall see by and by, a subject of considerable practical importance in the mechanic arts; and an accurate knowledge of the relation would be of great use also to the distiller; and it would be no less important to discover the relation of their elasticity and density, by examining their compressibility, in the same manner as we have ascertained the relation in

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* "Mr Dalton supposes the difference of the temperature of two vapours of the same elastic force, raised from two different liquids, to be a constant quantity; thus supposing alcohol to boil at 175°, and water at 212°, the constant difference is 37° belonging to those two liquids, and that the vapour of alcohol of 100° would be as elastic as that of water at 137°, &c. &c. On this principle the case supposed above could not happen; but it is consistent with the conclusion (Art. 14) as to principle, though not as to quantity, for 212° —70° = 142°." J. S.

† "These experiments were unknown to me at the time I invented the improved engine in 1765, and for long after; perhaps that invention was the cause of their being made." W.

the case of what we call *aerial fluids*, that is, such as we have never observed in the form of liquids or solids, except in consequence of their union with each other or with other bodies. In the article PNEUMATICS we shall take notice of it as something like a natural law, that all those airs, or gases, as they are now called, had their elasticity very nearly, if not exactly, proportional to their density. This appears from the experiments of Achard, of Fontana, and others, on vital air, inflammable air, fixed air, and some others. It gives us some presumption to suppose that it holds in all elastic vapours whatever, and that it is connected with their elasticity; and it renders it somewhat probable that they are all elastic, *only* because the cause of heat (the matter of fire if you will) is elastic, and that their law of elasticity, in respect of density, is the same with that of fire.

15. But it must be observed, that although we thus assign the elasticity of fire as the immediate cause of the elasticity of vapour, in the same way, and on the same grounds, that we ascribe the fluidity of brine to the fluidity of the water which holds the solid salt in solution, it does not follow that this is owing, as is commonly supposed, to a repulsion or tendency to recede from each other exerted by the particles of fire. We are as much entitled to infer a repulsion of unlimited extent between the particles of water; for we see that by its means a single particle of sea-salt becomes disseminated through the whole of a very large vessel. If water had not been a visible and a palpable substance, and the salt only had been visible and palpable, we might have formed a similar notion of chemical solution. But we, on the contrary, have considered the *quaquaversus* motion or expansion of the salt as a dissemination among the particles of water; and we have ascribed it to the strong attraction of the atoms of salt for the atoms of water, and the attraction of these last for each other, think-

ing that each atom of salt accumulates round itself a multitude of watery atoms, and by so doing must recede from the other saline atoms. Nay, we farther see, that by forces which we naturally consider as attractions, an expansion may be produced of the whole mass, which will act against external mechanical forces. It is thus that wood swells with almost insuperable force by imbibing moisture; it is thus that a sponge immersed in water becomes really an elastic compressible body; resembling a blown bladder; and there are appearances which warrant us to apply this mode of conception to elastic fluids.—When air is suddenly compressed, a thermometer included in it shews a rise of temperature; that is, an appearance of heat now redundant which was formerly combined. The heat seems to be squeezed out as the water from the sponge.

16. Accordingly this opinion, that the elasticity of steam and other vapours is owing merely to the attraction for fire, and the consequent dissemination of their particles through the whole mass of fire, has been entertained by many naturalists, and it has been ascribed entirely to attraction. We by no means pretend to decide; but we think the analogy by far too slight to found any confident opinion on it. The aim is to solve phenomena by attraction only, as if it were of more easy conception than repulsion. Considered merely as facts, they are quite on a par. The appearances of nature in which we observe actual recesses of the parts of body from each other, are as distinct, and as frequent and familiar, as the appearances of actual approach. And if we attempt to go farther in our contemplation, and to conceive the way and the forces by which either the approximation or recesses of the atoms are produced, we must acknowledge that we have no conception of the matter; and we can only say, that there is a cause of these motions, and we call it a force, as in every case of the production of motion. We call it attraction

or repulsion just as we happen to contemplate an access or a recess. But the analogy here is not only slight, but imperfect, and fails most in those cases which are most simple, and where we should expect it to be most complete. We can squeeze water out of a sponge, it is true, or out of a piece of green wood; but when the white of an egg, the tremella, or some gums, swell to a hundred times their dry dimensions by imbibing water, we cannot squeeze out a particle. If fluidity (for the reasoning must equally apply to this as to vapourousness) be owing to an accumulation of the extended matter of fire, which gradually expanded the solid by its very minute additions; and if the accumulation round a particle of ice, which is necessary for making it a particle of water, be so great in comparison of what gives it the expansion of one degree, as experiment obliges us to conclude—it seems an inevitable consequence, that all fluids should be many times rarer than the solids from which they are produced. But we know that the difference is trifling in all cases, and in some (water, for instance, and iron) the solid is rarer than the fluid.

17. Many other arguments (each of them perhaps of little weight when taken alone, but which are all systematically connected,) concur in rendering it much more probable that the matter of fire, in causing elasticity, acts immediately by its own elasticity, which we cannot conceive in any other way than as a mutual tendency in its particles to *recede* from each other; and we doubt not but that, if it could be obtained alone, we should find it an elastic fluid like air. We even think that there are cases in which it is observed in this state. The elastic force of gunpowder is very much beyond the elasticity of all the vapours which are produced in its deflagration, each of them being expanded as much as we can reasonably suppose by the great heat to which they are exposed. I exploded some gunpowder mixed with a considerable

portion of finely powdered quartz, and another parcel mixed with fine filings of copper. The elasticity was measured by the penetration of the ball which was discharged, and was great in the degree now mentioned. The experiment was so conducted, that much of the quartz and copper was collected; none of the quartz had been melted, and some of the copper was not melted. The heat, therefore, could not be such as to explain the elasticity by expansion of the vapours; and it became not improbable that fire was acting here as a detached chemical fluid by its own elasticity. But to return to our subject.

18. There is one circumstance in which we think our own experiments shew a remarkable difference (at least in degree) between the condensible and incondensible vapour. It is well known, that when air is very suddenly expanded, cold is produced, and heat when it is suddenly condensed. When making experiments with the hopes of discovering the connection between the elasticity and density of the vapours of boiling water, and also of boiling spirits of turpentine, we found the change of density accompanied by a change of temperature vastly greater than in the case of incoercible gases. When the vapour of boiling water was suddenly allowed to expand into five times its bulk, we observed the depression of a large and sensible air thermometer to be at least four or five times greater than in a similar expansion of common air of the same temperature. The chemical reader will readily see reasons for expecting, on the contrary, a smaller alteration of temperature, both on account of the much greater rarity of the fluid, and on account of a partial condensation of its water, and the consequent disengagement of combined heat.

19. This difference in the quantity of fire which is combined in vapours and gases is so considerable, as to authorise us to suppose that there is some difference in the

chemical constitution of vapours and gases, and that the connection between the specific bases of the vapour and the fire which it contains, is not the same in air, for instance, as in the vapour of boiling water; and this difference may be the reason why the one is easily condensible by cold, while the other has never been exhibited in a liquid or solid form, except by means of its chemical union with other substances. In this particular instance we know that there is an essential difference; that in vital or atmospheric air there is not only a prodigious quantity of fire which is not in the vapour of water, but that it also contains light, or the cause of light, in a combined state. This is fully evinced by the great discovery of Mr Cavendish of the composition of water. Here we are taught that water (and consequently its vapour) consists of air from which the light and greatest part of the fire have been separated. And the subsequent discoveries of the celebrated Lavoisier show, that almost all the condensible gases with which we are acquainted, consist either of airs which have already lost much of their fire (and perhaps light too), or of matters in which we have no evidence of fire or light being combined in this manner.

This consideration may go far in explaining this difference in the condensibility of these different species of aerial fluids, the gases and the vapours; and it is with this qualification only that we are disposed to allow that all bodies are condensible into liquids or solids by abstracting the heat. In order that vital air may become liquid or solid, we hold that it is not sufficient that a body be presented to it which shall simply abstract its heat. This would only abstract its uncombined fire. But another and much larger portion remains chemically combined by means of light. A chemical affinity must be brought into action which may abstract, not the fire from the oxygen (to speak the language of Mr Lavoisier), but the oxygen from the fire and light. And our production is not the detached basis of air, but detached heat and light, and the formation of an oxide of some kind.

To prosecute the chemical consideration of STEAMS farther than these general observations, which are applicable to all, would be almost to write a treatise of chemistry, and would be a repetition of many things which have been treated of in sufficient detail in other articles of this work. We shall therefore conclude this article with some other observations, which are also general, with respect to the different kinds of coercible vapours, but which have a particular relation to the following article.

20. Steam or vapour is an elastic fluid, whose elasticity balances the pressure of the atmosphere; and it has been produced from a solid or liquid body raised to a sufficient temperature for giving it this elasticity; that is, for causing the fluid to boil. This temperature must vary with the pressure of the air. Accordingly it is found, that when the air is light (indicated by the barometer being low), the fluid will boil sooner. When the barometer stands at 30 inches, water boils at the temperature 212°. If it stands so low as 28 inches, water will boil at 208½. In the plains of Quito, or at Gondair in Abyssinia, where the barometer stands at about 21 inches, water will boil at 195°. Highly rectified alcohol will boil at 160°, and vitriolic ether will boil at 88° or 89°. This is a temperature by no means uncommon in these places; nay, the air is frequently warmer. Vitriolic ether, therefore, is a liquor which can hardly be known in those countries. It is hardly possible to preserve it in that form. If a phial have not its stopper firmly tied down, it will be blown out, and the liquor will boil and be dissipated in steam. On the top of Chimboraço, the human blood must be disposed to form vapour or steam.

21. We said some time ago, that we had concluded,

from some experiments made in the receiver of an air-pump, that fluids boil *in vacuo* at a temperature nearly 120 degrees lower than that necessary for their boiling in the open air. But we now see that this must have been but a gross approximation; for in these experiments the fluids were boiling under the pressure of the vapour which they produced, and which could not be abstracted by working the pump. It appears from the experiments of Lord Charles Cavendish, (1760) mentioned in the article PNEUMATICS, that water of the temperature 72° was converted into elastic vapour, which balanced a pressure of ¾ths of an inch of mercury, and in this state it occupied the receiver, and did not allow the mercury in the gauge to sink to the level. As fast as this was abstracted by working the air-pump, more of it was produced from the surface of the water, so that the pressure continued the same, and the water did not boil. Had it been possible to produce a vacuum above this water, it would have boiled for a moment, and would even have continued to boil, if the receiver could have been kept very cold.

22. Upon reading these experiments, and some very curious ones of Mr Nairne, in the Phil. Trans. (1777,) I was induced to examine more particularly the relation between the temperature of vapour and its elasticity, in the following manner:

ABCD (Fig. 2.) is the section of a small digester made of copper. Its lid, which is fastened to the body with screws, is pierced with three holes, each of which had a small pipe soldered into it. The first hole was furnished with a brass safety-valve V, nicely fitted to it by grinding. The area of this valve was exactly ¼th of an inch. There rested on the stalk at the top of this valve the arm of a steel-yard carrying a sliding weight. This arm had a scale of equal parts, so adjusted to the weight, that the number on the scale corresponded to the inches of mercury, whose pressure on the under surface of the valve is equal to that of the steelyard on its top; so that when the weight was at the division 10, the pressure of the steelyard on the valve was just equal to that of a column of mercury 10 inches high, and ¼th of an inch base. The middle hole contained a thermometer T firmly fixed into it, so that no vapour could escape by its sides. The ball of this thermometer was but a little way below the lid. The third hole received occasionally the end of a glass pipe SGF, whose descending leg was about 36 inches long. When this syphon was not used, the hole was properly shut with a plug.

The vessel was half filled with distilled water which had been purged of air by boiling. The lid was then fixed on, having the third hole S plugged up. A lamp being placed under the vessel, the water boiled, and the steam issued copiously by the safety-valve. The thermometer stood at 213, and a barometer in the room at 29.9 inches. The weight was then put on the fifth division. The thermometer immediately began to rise; and when it was at 220, the steam issued by the sides of the valve. The weight was removed to the 10th division; but, before the thermometer could be distinctly observed, the steam was issuing at the valve. The lamp was removed farther from the bottom of the vessel, that the progress of heating might be more moderate; and when the steam ceased to issue from the valve, the thermometer was at 227. The weight was now shifted to 15; and, by gradually approaching the lamp, the steam again issued, and the thermometer was at 232½. This mode of trial was continued all the way to the 75th division of the scale. The experiments were then repeated in the contrary order; that is, the weight being suspended at the 75th division, and the steam issuing strongly at the valve, the lamp was withdrawn, and the moment the steam ceased to come out, the thermometer was observed. The same was done at the 70th, 65th division, &c. These experiments were several times repeated both ways; and the means of all the results for each division are expressed in the following table, where column 1st expresses the elasticity of the steam, being the sum of 29.9, and the division of the steelyard; column 2d expresses the temperature of the steam corresponding to this elasticity.

Elasticity.	*Temperature.*
35 inches.	219°
4	226
45	232
50	237
55	242
60	247
65	251
70	255
75	259
80	263
85	267
90	270½
95	274½
100	278
105	281

A very different process was necessary for ascertaining the elasticity of the steam in lower temperatures, and consequently under smaller pressures than that of the atmosphere. The glass syphon SGF was now fixed into its hole in the lid of the digester. The water was made to boil smartly for some time, and the steam issued copiously both at the valve and at the syphon. The lower end of the syphon was now immersed into a broad saucer of mercury, and the lamp instantly removed, and every thing was allowed to grow cold. By this the steam was gradually condensed, and the mercury rose in the syphon, without sensibly sinking in the saucer. The valve and all the joints were smeared with a thick clammy cement, composed of oil, tallow, and rosin, which effectually prevented all ingress of air. The weather was clear and frosty, and the barometer standing at 29.84, and the thermometer in the vessel at 42°. The mercury in the syphon stood at 29.7, or somewhat higher, thus showing a very complete condensation. The whole vessel was surrounded with pounded ice, of the temperature 32°. This made no sensible change in the height of the mercury. A mark was now made at the surface of the mercury. One observer was stationed at the thermometer, with instructions to call out as the thermometer reached the divisions 42, 47, 52, 57, and so on by every five degrees till it should attain the boiling heat. Another observer noted the corresponding descents of the mercury by a scale of inches, which had its beginning placed at 29.84 from the surface of the mercury in the saucer.

The pounded ice was now removed, and the lamp placed at a considerable distance below the vessel, so as to warm its contents very slowly. These observations being very easily made, were several times repeated, and their mean results are set down in the following table: Only observe, that it was found difficult to note down the descents for every fifth degree, because they succeeded each other so fast. Every 10th was judged sufficient for establishing the law of variation. The first column of the table contains the temperature, and the second the descent (in inches) of the mercury from the mark 29.84.

Temperature.	*Elasticity.*
32°	0.0
40	0.1
50	0.2
60	0.35
70	0.55
80	0.82
90	1.18
100	1.61
110	2.25
120	3.00
130	3.95
140	5.15
150	6.72
160	8.65
170	11.05
180	14.05
190	17.85
200	22.62
210	28.65

Four or five numbers at the top of the column of elasticities are not so accurate as the others, because the mercury passed pretty quickly through these points. But the progress was extremely regular through the remaining points; so that the elasticities corresponding to temperatures above 70° may be considered as very accurately ascertained.

Not being altogether satisfied with the method employed for measuring the elasticity in temperatures above that of boiling water, a better form of experiment was adopted. (Indeed it was the want of other apparatus which made it necessary to employ the former.) A glass tube was procured of the form represented in Fig. 3. having a little cistern L, from the top and bottom of which proceeded the syphons K and MN. The cistern contained mercury, and the tube MN was of a slender bore, and was about six feet two inches long. The end K was firmly fixed in the third hole of the lid, and the long leg of the syphon was furnished with a scale of inches, and firmly fastened to an upright post.

The lamp was now applied at such a distance from the vessel as to warm it slowly, and make the water boil, the steam escaping for some time through the safety-valve. A heavy weight was then suspended on the steelyard; such as it was known that the vessel would support, and, at the same time, such as would not allow the steam to force the mercury out of the long tube. The thermometer began immediately to rise, as also the mercury in the tube MN. Their correspondent stations are marked in the following table:—

Temperature.	*Elasticity.*
212°	0.0
220	5.9
230	14.6
240	25.0
250	36.9
260	50.4
270	64.2
280	76.0

This form of the experiment is much more susceptible of accuracy than the other, and the measures of elasticity are more to be depended on. In repeating the experiment, they were found much more constant; whereas, in the former method, differences occurred of two inches and upwards.

We may now connect the two sets of experiments into one table, by adding to the numbers in this last table the constant height 29.9, which was the height of the mercury in the barometer during the last set of observations.

Temperature.	*Elasticity.*
32°	0.0
40	0.1
50	0.2
60	0.35
70	0.55
80	0.82
90	1.18
100	1.6
110	2.25
120	3.0
130	3.95
140	5.15
150	6.72
160	8.65
170	11.05
180	14.05
190	17.85
200	22.62
210	28.65
220	35.8
230	44.5
240	54.9
250	66.8
260	80.3
270	94.1
280	105.9 *

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* "Experiments on this subject have been published by Mr De Betancourt, (see Prony Arch. Hydraulique) by Mr Schmidt, and by Mr Dalton, (see Manchester Memoirs).

"In the winter 1764-5, I made experiments at Glasgow on the subject, in the course of my endeavours to improve the steam-engine, and as I did not then think of any *simple* method of trying the elasticities of steam at temperatures less than that of boiling water, and had at hand a digester by which the elasticities at greater heats could be tried, I considered that, by establishing the ratios in which they proceeded, the elasticities at lower heats might

33. In the Memoirs of the Royal Academy of Berlin for 1782, there is an account of some experiments made by Mr Achard on the elastic force of steam, from the

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be found nearly enough for my purpose. I therefore fitted a thermometer to the digester with its bulb in the inside, placed a small cistern with mercury also within the digester, fixed a small barometer tube with its end in the mercury, and left the upper end open. I then made the digester boil for some time, the steam issuing at the safety-valve, until the air contained in the digester was supposed to be expelled. The safety-valve being shut, the steam acted upon the surface of the mercury in the cistern, and made it rise in the tube. When it reached to 15 inches above the surface of the mercury in the cistern, the heat was 236°; and at 30 inches above that surface, the heat was 252°. Here I was obliged to stop, as I had no tube longer than 34 inches, and there was no white glass made nearer than Newcastle upon Tyne. I therefore sealed the upper end of the tube hermetically whilst it was empty, and when it was cool immersed the lower end in the mercury, which now could only rise in the tube by compressing the air it contained. The tube was somewhat conical; but by ascertaining how much it was so, and making allowances accordingly, the following points were found, which, though not exact, were tolerably near for an apperçu. At 29½ inches (with the sealed tube) the heat was 252°, at 75½ inches the heat was 264°, and at 110½ inches 292°. (That is, after making allowances for the pillar of mercury supported, and the pillar which would be necessary to compress the air into the space which it occupied, these were the results). From these elements I laid down a curve, in which the abscissæ represented the temperatures, and the ordinates the pressures, and thereby found the law by which they were governed, sufficiently near for my then purpose. It was not till the years 1773-4, that I found leisure to make further experiments on this subject, of which, though I do not consider the results as accurate, I shall give an account here, as they were in point of date prior to any others that I was then acquainted with.

" A tin pan of about five inches in diameter, and four inches deep, had a hole made in its bottom, near one side, and in this hole was soldered a socket somewhat conical, which nearly fitted a barometer tube with which the experiments were to be made. This tube was about 36 inches long, and had a ball at one end about 1½ inches diameter, the contents of which were nearly equal to those of the stem of the tube; some paper was lapped round the tube near the ball, and it was forced tight into the conical socket of the pan, so that the ball was within the latter, at such a height that it might be immersed in water. The tube and pan were then inverted, and the ball was filled with clean mercury, and the stem with distilled water fresh boiled. The tube was re-inverted, so that the ball and pan were uppermost; the lower temperature 32° to 212°. They agree extremely well with those mentioned here, rarely differing more than two

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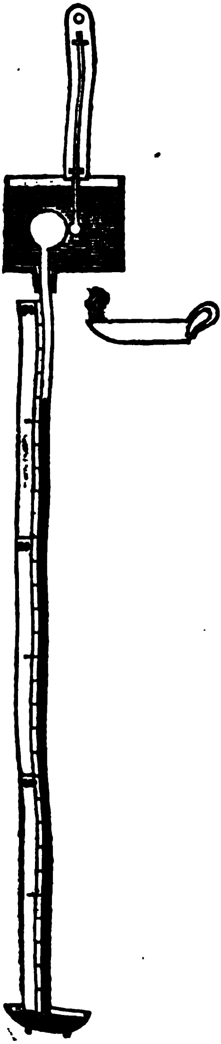

end of the tube being shut by the finger, the water ascended into the ball, and the mercury occupied the tube. The lower end of the latter being then placed in a cistern of mercury, and released from the finger, the mercury and water descended, and the ball was left partly empty: being agitated in this position, and let stand some time, much air was extricated from the water; the tube was inclined as much as it could be, and again inverted, the air let out, and its place supplied with boiling water. It was again placed with the ball uppermost, the end of the tube stopt, the pan filled with hot water which was made to boil by means of a lamp, the lower end of the tube being placed in the cistern, and released from the finger, the mercury descended into the cistern, but upon the water in the pan being suffered to cool, partly rose again into the tube. Much air was thus liberated, and more was got rid of by agitation, in the manner of the water-hammer, and by leaving it standing for some time erect, until at last I got it so free from air, that when I raised it upright, it supported a column of mercury 34 inches high; and no vacuum was formed until it was violently shaken, when it fell down suddenly and settled at 28.75 inches, but upon being inclined, a speck of air always remained, though, when it was expanded by a pillar of mercury 27 inches high, this speck was not larger than a pin's head.*

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* "I was much assisted in the means of freeing the water from air by Mr De Luc's very valuable Treatise upon the Modifications of the Atmosphere, which came to my hands about this time; but I had not the pleasure of meeting the author until long afterwards, when we commenced a friendship which has continued uninterrupted to the present time." W.

or three-tenths of an inch. He also examined the elasticity of the vapour produced from alcohol, and found, that when the elasticity was equal to that of the vapour of water, the temperature was about 35° lower. Thus, when the elasticity of both was measured by 28.1 inches of mercury, the temperature of the watery vapour was 209°, and that of the spirituous vapour was 173°. When

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" In this state, when the tube was perpendicular, I found the mercury to stand at 28.75 inches, the column of water above it was about 6¾ inches, = half an inch of mercury. The whole then being 29.25 inches when the stationary barometer stood at 29.4, the difference, or pillar supported by the elasticity of the steam = 0.15 inch. The water in the pan was then heated exceedingly slowly by a lamp, and stirred continually by a feather to make the heat as equal as possible. The results are shewn in the following Table.

TABLE, No. I.

Heat.	Elasticities.	Heats.	Elasticities.	Heats.	Elasticities.	Heats.	Elasticities.
°	Inches.	°	Inches.	°	Inches.	°	Inches.
55	0.15	135	4.53	167	11.07	187	17.51
74	0.65	142	5.46	172	11.95	189	18.45
81	0.80	148	6.40	175	12.88	191	19.38
95	1.30	153	7.325	177.5	13.81	193.5	20.34
104	1.75	157	8.25	180	14.73	196.5	21.26
118	2.68	161	9.18	182.5	15.66		
128	3.60	164	10.10	185	16.58		

" At this time (1774) I tried a set of experiments in the same manner on a saturated solution of common salt. When this solution was perfectly saturated by boiling, and was put into the tube, it precipitated a quantity of salt which disturbed the experiment. I was therefore obliged to take it out, and filter it, during which process it attracted moisture from the air, and appeared by its boiling point not to be perfectly saturated. Though it was more free from air than water is, yet it parted from what it contained with great difficulty, and would part with none when shaken as a water-hammer, though it opened in all parts of the liquor. The result of this experiment is contained in the annexed Table.

the elasticity was 18.5, the temperature of the water was 189.5, and that of the alcohol 154.6. When the elasticity

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STATIONARY BAROMETER, 29,5.

TABLE, No. II.

Heats.	Elasticities.	Heats.	Elasticities.	Heats.	Elasticities.	Heats.	Elasticities.
	Inches.		Inches.		Inches.		Inches.
46°	0.01	154	5.36	187°	12.67	208°	20.86
76	0.36	160	6.27	193.5	14.5	210	21.8
85	0.58	165	7.2	195.5	15.34	212	22.74
92	0.81	169	8.12	198.5	16.25	214	23.66
113	1.72	173	9.08	201.5	17.16	216	24.6
129	2.63	177	9.94	203.5	18.1	218	25.52
139	3.54	180	10.85	205.5	19.08	220	26.5
147	4.45	183	11.76	207	19 94		

In the same manner I tried a set of experiments upon spirit of wine, the results of which are contained in the annexed Table.

TABLE, No. III.

Heats.	Elasticities.	Heats.	Elasticities.	Heats.	Elasticities.	Heats.	Elasticities.
	Inches.		Inches.		Inches.		Inches.
34°	0.22	120°	7.12	146.5°	15.09	164°	22.59
40	0.929	124,5	8.46	148.5	15.974	166	23.53
67	1.897	128	9.4	151	16.908	167	24.47
84	2.806	132	10.34	152.5	17.85	168	25.4
95	3.744	135	11.32	155	18.8	169	26.35
103	4.728	139	12.21	157	19.75	171	27.3
110	5.63	141.5	13.15	160	20.71	Stat. Bar. 29·4	
114	6.58	144	14.1	162.5	21.65		

" Upon considering the probable cause of the difference, especially in the lower heats, between my experiments and these of Mr Southern, related in his letter annexed to this essay, I can only reconcile them by supposing that the stationary barometer with which the comparison was made, had its scale was 11.05, the water was 168°, and the alcohol 134°.4. Observing the difference between the temperature of equal-

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placed 0.2 of an inch too low, and by adding that quantity to the elasticities in Table 1st, they approach nearly to Mr Southern's experiments.

"If that conjecture is adopted, the same addition will be necessary to Tables 2d and 3d, as they were compared with the same stationary barometer.

"To determine the heats at which water boils when pressed by columns of mercury above 30 inches, a tube of 55 inches long was employed; one end was put through a hole in the cover of a digester, and made tight by being lapped round with paper, and within the digester the end of the tube was immersed in a cistern of mercury. A thermometer was fixed in another opening, so that the bulb was in the inside of the digester, and the stem and scale without; and the bulb was kept half an inch from the cover of the digester by a wooden collar. The cover being fixed on tight, and the digester half filled with water, it was heated by means of a large lamp.

"The air in the upper part of the digester expanding by heat, the column of mercury in the tube was considerably raised by that expansion before the water boiled. The air was let out, and the water heated to boiling; still, however, some air remained, for the mercury stood at 213°½. That deduction being made, the following Table shows the heats and corresponding elasticities.

Heats.	Elasticities.	Heats.	Elasticities.	Heats.	Elasticities.	Heats.	Elasticities.
213	30	228	39	240°	49	259°	66
215	31	229.5	40	242.5	50	261	68
217	32	231	41	244.5	52	262.5	70
219	33	232.5	42	247	54	264.5	72
220.5	34	234	43	248.5	56	266.5	74
222	35	235	44	250.5	58	268	76
223.5	36	236.5	45	252.5	60	269.5	78
225	37	237.5	46	255	62	271	80
226.5	38	238.5	47	257	64	272.5	82

"In making these experiments, the digester was heated very slowly, and the heat was kept stationary as much as was possible at each observation, so that the whole series occupied some hours. The degrees of elasticity were observed by my friend Dr Irvine, whilst I observed those of the thermometer in all these experiments.

"With the whole of the observations, I was, after all, by no means satisfied, as I perceived there were irregularities in the results which my more urgent avocations did not permit me to explore the causes of and to correct.

ly elastic vapours of water and alcohol not to be constant, but gradually to diminish, in Mr Achard's experiments, along with the elasticity, it became interesting to discover whether, and at what temperature, this difference would vanish altogether. Experiments were accordingly made by the writer of this article, similar to those made with water. They were not made with the same scrupulous care, nor repeated as they deserved, but they furnished rather an unexpected result. The following table will give the reader a distinct notion of them:

Temperature.	*Elasticity.*
32°	0.0
40	0.1
60	0.8
80	1.8
100	3.9
120	6.9
140	12.2
160	21.3
180	34.
200	52.4
220	78.5
240	116.

"The matter remained in that state till 1796, when I requested Mr Southern to try them over again, in the performance of which he was assisted by Mr William Creighton. The results of these observations are contained in Mr Southern's letter to me, which follows this memoir; and, from the very great care with which the experiments were made, the known accuracy of both Mr Southern and Mr Creighton, and the agreement of the experiments with one another, I have reason to believe them as nearly perfect as the subject admits of. The method he adopted of trying the elasticities above the temperature of boiling water by a piston, accurately fitted to a cylinder, is much to be preferred to that adopted by Dr Robison, and is more manageable under great elasticities than that of a long pillar of mercury." W.

24. We say that the result was unexpected; for as the natural boiling point seemed by former experiments to be in all fluids about 120°, or more, below their boiling point in the ordinary pressure of the atmosphere, it was reasonable to expect that the temperature at which they ceased to emit sensibly elastic steam would have some relation to their temperatures when emitting steam of any determinate elasticity. Now as the vapour of alcohol of elasticity 30 has its temperature about 36° lower than the temperature of water equally elastic, it was to be expected that the temperature at which it ceased to be sensibly affected would be several degrees lower than 32°. It is evident, however, that this is not the case. But this is a point that deserves more attention, because it is closely connected with the chemical relation between the element (if such there be) of fire and the bodies into whose composition it seems to enter as a constituent part. What is the temperature 32°, to make it peculiarly connected with elasticity? It is a temperature assumed by us for our own conveniency, on account of the familiarity of water in our experiments. Ether, we know, boils in a temperature far below this, as appears from Dr Cullen's experiments narrated in the Essays Physical and Literary of Edinburgh. On the faith of former experiments, we may be pretty certain that it will boil in vacuo at the temperature—14°, because in the air it boils at +106°. Therefore we may be certain, that the steam or vapour of ether, when of the temperature 32°, will be very sensibly elastic. Mr Lavoisier says, that if it be exposed in an exhausted receiver in winter, its vapour will support mercury at the height of 10 inches. A series of experiments on this vapour, similar to the above, would be very instructive. We even wish that those on alcohol were more carefully repeated. If we draw a curve line, of which the abscissa is the line of temperatures, and the ordinates are the corresponding heights of the mercury in these experiments on water and alcohol, we shall observe,

that although they both sensibly coincide at 32°, and have the abscissa for their common tangent, a very small error of observation may be the cause of this, and the curve which expresses the elasticity of spirituous vapour may really intersect the other, and go backwards considerably beyond 32°.

25. This range of experiments gives rise to some curious and important reflections. We now see that no particular temperature is necessary for water assuming the form of permanently elastic vapour, and that it is highly probable that it assumes this form even at the temperature 32°; only its elasticity is too small to afford us any sensible measure. It is well known that even ice evaporates (see experiments to this purpose by Mr Wilson, in the Philosophical Transactions, when a piece of polished metal, covered with hoar-frost, became perfectly clear by exposing it to a dry frosty wind.).

Even mercury evaporates, or is converted into elastic vapour, when all external pressure is removed. The dim film which may frequently be observed in the upper part of a barometer which stands near a stream of air, is found to be small globules of mercury sticking to the inside of the tube. They may be seen by the help of a magnifying glass, and are the best test of a well-made barometer. They will be entirely removed by causing the mercury to rise along the tube. It will lick them all up. They consist of mercury which had evaporated in the void space, and was afterwards condensed by the cold glass. But the elasticity is too small to occasion a sensible depression of the column, even when considerably warmed by a candle.

26. Many philosophers accordingly imagine, that spontaneous evaporation in low temperatures is produced in this way. But we cannot be of this opinion, and must still think that this kind of evaporation is produced by the dissolving power of the air. When moist air is sudenly rarefied, there is always a precipitation of water. This is most

distinctly seen when we work an air-pump briskly. A mist is produced, which we see plainly fall to the bottom of the receiver. But, by this new doctrine, the very contrary should happen, because the tendency of water to appear in the elastic form is promoted by removing the external pressure; and we really imagine that more of it now actually becomes simple elastic watery vapour. But the mist or precipitation shows incontrovertibly, that there had been a previous solution. Solution is performed by forces which act in the way of attraction; or, to express it more safely, solutions are accompanied by the mutual approaches of the particles of the menstruum and solvend: all such tendencies are *observed* to increase by a diminution of distance. Hence it *must* follow, that air of double density will dissolve more than twice as much water. Therefore, when we suddenly rarefy saturated air (even though its heat should not diminish) some water must be let go. What may be its quantity we know not; but it *may be more* than what would now become elastic by this diminution of surrounding pressure; and it is not unlikely but this may have some effect in producing the vesicles which we found so difficult to explain. These may be filled with pure watery vapour, and be floating in a fluid composed of water dissolved in air. An experiment of Fontana's seems to put this matter out of doubt. A distilling apparatus AB (fig. 4.) was so contrived, that the heat was applied above the surface of the water in the alembic A. This was done by inclosing it in another vessel CC, filled with hot water. In the receiver B there was a sort of barometer D, with an open cistern, in order to see what pressure there was on the surface of the fluid. While the receiver and alembic contained air, the heat applied at A produced no sensible distillation during several hours; but on opening a cock E in the receiver at its bottom, and making the water in the alembic to boil, steam was produced which soon expelled all the air, and followed it through the cock. The cock was now shut, and the whole allowed to grow cold by removing the fire, and applying cold water to the alembic. The barometer fell to a level nearly; then warm water was allowed to get into the outer vessel CC. The barometer rose a little, and the distillation went on briskly without the smallest ebullition in the alembic. The conclusion is obvious: while there was air in the receiver and communicating pipe, the *distillation* proceeded entirely by the dissolving power of this air. Above the water in the alembic it was quickly saturated; and this saturation proceeded slowly along the still air in the communicating pipe, and at last might take place through the whole of the receiver. The sides of the receiver being kept cold, should condense part of the water dissolved in the air in contact with them, and this should trickle down the sides and be collected. But any person who has observed how long a crystal of blue vitriol will lie at the bottom of a glass of still water before the tinge will reach the surface, will see that it must be next to impossible for distillation to go on in these circumstances; and accordingly none was observed. But when the upper part of the apparatus was filled with pure watery vapour, it was supplied from the alembic as fast as it was condensed in the receiver, just as in the pulse-glass.

27. Another inference which may be drawn from these experiments is, that Nature seems to affect a certain law in the dilatation of aeriform fluids by heat. They seem to be dilatable nearly in the proportion of their present dilatation. For if we suppose that the vapours resemble air, in having their elasticity in any given temperature proportional to their density, we must suppose that if steam of the elasticity 60, that is, supporting 60 inches of mercury, were subjected to a pressure of 30 inches, it would expand into twice its present bulk. The augmentation of elasticity therefore is the measure of the bulk into which it would expand in order to acquire its former elasticity. Taking the increase of elasticity, therefore, as a measure of the bulk into which it

would expand under one constant pressure, we see that equal increments of temperature produce nearly equal multiplications of bulk. Thus, if a certain diminution of temperature diminishes its bulk ¼th, another equal diminution of temperature will diminish this new bulk ¼th very nearly. Thus, in our experiments, the temperatures 110°, 140°, 170°, 200°, 230°, are in arithmetical progression, having equal differences; and we see that the corresponding elasticities, 2.25, 5.15, 11.05, 22.62, 44.7, are very nearly in the continued proportion of 1. to 2. The elasticity corresponding to the temperature 260, deviates considerably from this law, which would give 88 or 89 instead of 80; and the deviation increases in the higher temperatures. But still we see that there is a considerable approximation to this law; and it will frequently assist us to recollect, that whatever be the present temperature, an increase of 30 degrees doubles the elasticity and the bulk of watery vapour.

That 4° will increase the elasticity from				1 to 1 $\frac{1}{10}$
8	-	-	-	1 to 1 $\frac{1}{5}$
10		-	-	1 to 1 $\frac{1}{4}$
12½		-	-	1 to 1 $\frac{1}{3}$
18	-	-	-	1 to 1 $\frac{1}{4}$
22		-	-	1 to 1 $\frac{2}{3}$
24		-	-	1 to 1 $\frac{3}{4}$
26	-	-	-	1 to 1 $\frac{4}{5}$

This is sufficiently exact for most practical purposes. Thus an engineer finds that the injection cools the cylinder of a steam-engine to 192°. It therefore leaves a steam whose elasticity is three-fifths of its full elasticity, = 18 inches of Mercury. But it is better at all times to have recourse to the table. Observe, too, that in the lower temperatures, i. e. below 110°, this increment of temperature does more than double the elasticity.

28. This law obtains more remarkably in the incoercible vapours, such as vital air, atmospheric air, fixed air, &c. all of which have also their elasticity proportional to their bulk inversely; and perhaps the deviation from the law in steams is connected with their chemical difference of constitution. If the bulk were always augmented in the same proportion by equal augmentations of temperature, the elasticities would be accurately represented by the ordinates of a *logarithmic* curve, of which the temperatures are the corresponding abscissæ; and we might contrive such a scale for our thermometer, that the temperatures would be the common logarithms of the elasticities, or of the bulks having equal elasticity; or, with our present scale, we may find such a multiplier m for the number x of degrees of our thermometer (above that temperature where the elasticity is equal to unity), that this multiple shall be the common logarithm of the elasticity y; so that $mx = \log. y$.

But our experiments are not sufficiently accurate for determining the temperature where the elasticity is measured by 1 inch; because in these temperatures the elasticities vary by exceedingly small quantities. But if we take 11.05 for the unit of elasticity, and number our temperature from 170°, and make $m = 0.010035$, we shall find the product mx to be very nearly the logarithm of the elasticity. The deviations, however, from this law, are too great to make this equation of any use. But it is very practicable to frame an equation which shall correspond with the experiments to any degree of accuracy; and it has been done for air in a translation of General Roy's measurement of the Base at Hounslow Heath, into French, by Mr Prony. It is as follows: Let x be the degrees of Reaumur's thermometer; let y be the expansion of 10,000 parts of air; let e be $= 10$, $m = 2.7979$, $n = 0.01768$: then $y = e^{m+nx} - 627.5$. Now e being $= 10$, it is plain that e^{m+nx} is the number, of which $m+nx$ is the common logarithm. This formula is very exact as far as the temperature 60; but beyond this it needs a correction, because air, like the vapour of water, does not expand in the exact proportion of its bulk.

29. We observe this law considerably approximated to in the augmentation of the bulk or elasticity of elastic vapours; that is, it is a fact that a given increment of temperature makes very nearly the same proportional augmentation of bulk and elasticity. This gives us some notion of the manner in which the supposed expanding cause produces the effect. When vapour of the bulk 4 is expanded into a bulk 5 by an addition of 10 degrees of sensible heat, a certain quantity of fire goes into it, and is accumulated round each particle, in such a manner that the temperature of each, which formerly was *m*, is now $m+10$. Let it now receive another equal augmentation of temperature. This is now $m+20$, and the bulk is $\frac{5\times5}{4}$ or $6\frac{1}{4}$, and the arithmetical increase of bulk is $1\frac{1}{4}$. The absolute quantity of fire which has entered it is greater than the former, both on account of the greater augmentation of space and the greater temperature. Consequently if this vapour be compressed into the bulk 5, there must be heat or fire in it which is not necessary for the temperature $m+20$, far less for the temperature $m+10$. It must therefore emerge, and be disposed to enter a thermometer which has already the temperature $m+20$: that is, the vapour must grow hotter by compression; not by squeezing out the heat like water out of a sponge, but because the law of attraction for heat is deranged. It would be a very valuable acquisition to our knowledge to learn with precision the quantity of sensible heat produced in this way; but no satisfactory experiments have yet been made. M. Lavoisier, with his chemical friends and colleagues, were busily employed in this inquiry; but the wickedness of their countrymen deprived the world of this and many other important additions which we might have expected from this celebrated and unfortunate philosopher. He had made, in conjunction with M. de

in Place, a numerous train of accurate and expensive experiments for measuring the quantity of latent or combined heat in elastic vapours. This is evidently a very important point to the distiller and practical chemist. This heat must all come from the fuel; and it is greatly worth while to know whether any saving may be made of this article. Thus we know that distillation will go on either under the pressure of the air, or in an alembic and receiver from which the air has been expelled by steam; and we know that this last may be conducted in a very low temperature, even not exceeding that of the human body. But it is uncertain whether this may not employ even a greater quantity of fuel, as well as occasion a great expence of time. We are disposed to think, that when there is no air in the apparatus, and when the condensation can be speedily performed, the proportion of fuel expended to the fluid which comes over, will diminish continually as the heat, and consequently the density of the steam is augmented, because in this case the quantity of combined heat must be less. In the mean time, we earnestly recommend the trial of this mode of distillation in vessels cleared of air. It is undoubtedly of great advantage to be able to work with smaller fires, and it would secure us against all accidents of blowing off the head of the still, often attended with terrible consequences.

We must not conclude this article without taking notice of some natural phenomena which seem to owe their origin to the action of elastic steam.

The wonderful appearances of the Geyzer spring, in Iceland, are undoubtedly produced by the expansion of steam in ignited caverns. Of these appearances we suppose the whole train to be produced as follows.

30. A cavern may be supposed of a shape analogous to CBDEF (fig. 5.), having a perpendicular funnel AB issuing from a depressed part of the roof. The part F may

be lower than the rest, remote, and red-hot. Such places we know to be frequent in Iceland. Water may be continually trickling into the part CD. It will fill it up to B, and even up to E *e*, and then trickle slowly along into F. As soon as any gets into contact with an ignited part, it expands into elastic steam, and is partly condensed by the cold sides of the cavern, which it gradually warms, till it condenses no more. This production of steam hinders not in the smallest degree the trickling of more water into F, and the continual production of more steam. This now presses on the surface of the water in CD, and causes it to rise gradually in the funnel BA; but slowly, because its cold surface is condensing an immense quantity of steam. We may easily suppose that the water trickles faster into F than it is expended in the production of steam; so that it reaches farther into the ignited part, and may even fall in a stream into some deeper pit highly ignited. It will now produce steam in vast abundance, and of prodigious elasticity; and at once push up the water through the funnel in a solid jet, and to a great height. This must continue till the surface of the water sinks to BD. If the lower end of the funnel have any inequalities or notches, as is most likely, the steam will get admission along with the water, which in this particular place is boiling hot, being superficial, and will get to the mouth of the funnel, while water is still pressed in below. At last the steam gets in at B on all sides; and as it is converging to B, along the surface of the water, with prodigious velocity, it sweeps along with it much water, and blows it up through the funnel with great force. When this is over, the remaining steam blows out unmixed with water, growing weaker as it is expended, till the bottom of the funnel is again stopped by the water increasing in the cavern CBD. All the phenomena above ground are perfectly conformable to the necessary consequences of this very probable construction of the cavern. The feeling of being lifted up, immediately before the jet, in all probability is owing to a real heaving up of the whole roof of the cavern by the first expansion of the great body of steam. We had an accurate description of the phenomena from persons well qualified to judge of these matters, who visited these celebrated springs in 1789.

STEAM-ENGINE.

STEAM-ENGINE, is the name of a machine which derives its moving power from the elasticity and condensibility of the steam of boiling water. It is the most valuable present which the arts of life have ever received from the philosopher. The mariner's compass, the telescope, gunpowder, and other most useful servants to human weakness and ingenuity, were the productions of chance, and we do not exactly know to whom we are indebted for them; but the steam-engine was, in the very beginning, the result of reflection, and the production of a very ingenious mind; and every improvement it has received, and every alteration in its construction and principles, were also the results of philosophical study.

1. The steam-engine was beyond all doubt first invented by the marquis of Worcester during the reign of Charles II. This nobleman published in 1663 a small book, entitled "A Century of Inventions," giving some obscure and enigmatical account of an hundred discoveries or contrivances of his own, which he extols as of great importance to the public.* He appears to have been a person of much knowledge and great ingenuity: but his description or accounts

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* "In Hero of Alexandria's 'Spiritalia," there are two toys moving by steam, described in propositions 50 and 71. They are both moved on the principle of Barker's mill, by steam issuing from an eolopile, moveable round a centre or axis, and not of a nature to be of any real use." W.

of these inventions seem not so much intended to instruct the public, as to raise wonder; and his encomiums on their utility and importance are to a great degree extravagant, resembling more the puff of an advertising tradesman than the patriotic communications of a gentleman. The marquis of Worcester was indeed a projector, and very importunate and mysterious withal in his applications for public encouragement. His account, however, of the steam-engine, although by no means fit to give us any distinct notions of its structure and operation, is exact as far as it goes, agreeing precisely with what we now know of the subject. It is No. 68. of his inventions. His words are as follow "This admirable method which I propose of raising water by the force of fire has no bounds if the vessels be strong enough: for I have taken a cannon, and having filled it three-fourths full of water, and shut up its muzzle and touch-hole, and exposed it to the fire for 24 hours, it burst with a great explosion. Having afterwards discovered a method of fortifying vessels internally, and combined them in such a way that they filled and acted alternately, I have made the water spout in an uninterrupted stream 40 feet high; and one vessel of rarefied water raised 40 of cold water. The person who conducted the operation had nothing to do but turn two cocks; so that one vessel of water being consumed, another begins to force, and then to fill itself with cold water, and so on in succession."

2. It does not appear that the noble inventor could ever interest the public by these accounts. His character as a projector, and the many failures which persons of this turn of mind daily experience, probably prejudiced people against him, and prevented all attention to his projects. It was not till towards the end of the century, when experimental philosophy was prosecuted all over Europe with uncommon ardour, that these notions again engaged attention. Captain Savary, a person also of great ingenuity and ardent mind, saw the reality and practicability of the

marquis of Worcester's project. He knew the great expansive power of steam, and had discovered the inconceivable rapidity with which it is reconverted into water by cold; and he soon contrived a machine for raising water, in which both of these properties were employed. He says, that it was entirely his own invention. Dr Desaguliers insists that he only copied the marquis's invention, and charges him with gross plagiarism, and with having bought up and burned the copies of the marquis's book, in order to secure the honour of the discovery to himself. This is a very grievous charge, and should have been substantiated by very distinct evidence. Desaguliers produces none such; and he was much too late to know what happened at that time. The argument which he gives is a very foolish one, and gave him no title to consider Savary's experiment as a falsehood; for it might have happened precisely as Savary relates, and not as it happened to Desaguliers. The fact is, that Savary obtained his patent of invention after a hearing of objections, among which the discovery of the marquis of Worcester was not mentioned; and it is certain that the account given in the "Century of Inventions" could instruct no person who was not sufficiently acquainted with the properties of steam to be able to invent the machine himself.

8. Captain Savary obtained his patent *after having actually erected* several machines, of which he gave a description in a book entitled "The Miner's Friend," published in 1696, and in another work published in 1699. Much about this time, Dr Papin, a Frenchman and fellow of the Royal Society, invented a method of dissolving bones and other animal solids in water, by confining them in close vessels, which he called DIGESTERS, so as to acquire a great degree of heat. For it must be observed in this place, that it had been discovered long before (in 1684) by Dr Hooke, the most inquisitive experimental philosopher of that inquisitive age, that water could not be made to acquire above a

certain temperature in the open air; and that as soon as it begins to boil, its temperature remains fixed, and an increase of heat only produces a more violent ebullition, and a more rapid waste. But Papin's experiments made the elastic power of steam very familiar to him; and when he left England and settled as professor of mathematics at Marpurgh, he made many awkward attempts to employ this force in mechanics, and even for raising water. It appears that he had made experiments with this view in 1698, by order of Charles, landgrave of Hesse. For this reason the French affect to consider him as the inventor of the steam-engine. He indeed published some account of his invention in 1707; but he acknowledges that Captain Savary had also, and without any communication with him, invented the same thing. Whoever will take the trouble of looking at the description which he has given of these inventions, which are to be seen in the *Acta Eruditorum Lipsiæ,* and in Leupold's *Theatrum Machinarum,* will see that they are most awkward, absurd, and impracticable. His conceptions of natural operations were always vague and imperfect, and he was neither philosopher nor mechanician.

We are thus anxious about the claim of those gentlemen, because a most respectable French author, Mr Bossut, says in his *Hydrodynamique,* that the first notion of the steam-engine was *certainly* owing to Dr Papin, who had not only invented the digester, but had, in 1695, published a little performance describing a machine for raising water, in which the pistons are moved by the vapour of boiling water alternately dilated and condensed. Now the fact is, that Papin's first publication was in 1707, and his piston is nothing more than a floater on the surface of the water, to prevent the waste of steam by condensation; and the return of the piston is not produced, as in the steam-engine, by the condensation of the steam, but by admitting the air and a column of water to press it back into its place. The whole contrivance is so awkward, and so unlike any distinct notions of the subject, that it cannot do credit to any person.

4. We may add, that much about the same time, Mr Amontons contrived a very ingenious but intricate machine, which he called a *fire-wheel.* It consisted of a number of close buckets placed in the circumference of a wheel, and communicating with each other by very intricate circuitous passages. One part of this circumference was exposed to the heat of a furnace, and another to a stream or cistern of cold water. The communications were so disposed, that the steam produced in the buckets on one side of the wheel drove the water into buckets on the other side, so that one side of the wheel was always much heavier than the other; and it must therefore turn round, and may execute some work. The death of the inventor, and the intricacy of the machine, caused it to be neglected. Another member of the Parisian academy of sciences (Mr Deflandes) also presented to the academy a project of a steam-wheel, where the impulsive force of the vapour was employed; but it met with no encouragement. The English engineers had by this time so much improved Savary's first invention, that it supplanted all others. We have therefore no hesitation in giving the honour of the first and complete invention to the marquis of Worcester; and we are not disposed to refuse Captain Savary's claim to originality as to the construction of the machine, and even think it probable that his own experiments made him see the whole, independent of the marquis's account.*

Captain Savary's engine, as improved and simplified by himself, is as follows.

5. A (fig. 6.) represents a strong copper boiler properly

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* "It does not appear that the marquis of Worcester knew any thing of the use of an injection, as the machine described by him operated only by the expansive force of steam; whereas the injection was used in Savary's engine from the beginning, and is in all probability his invention." W.

built up in a furnace. There proceeds from its top a large steam-pipe B, which enters into the top of another strong vessel R, called the RECEIVER. This pipe has a cock at C, called the STEAM-COCK. In the bottom of the receiver is a pipe F, which communicates sidewise with the rising pipe KGH. The lower end H of this pipe is immersed in the water of the pit or well, and its upper part K opens into the cistern into which the water is to be delivered. Immediately below the pipe of communication F there is a valve G, opening when pressed from below, and shutting when pressed downwards. A similar valve is placed at I, immediately above the pipe of communication. Lastly, there is a pipe ED, which branches off from the rising pipe, and enters into the top of the receiver. This pipe has a cock D, called the INJECTION-COCK. The mouth of the pipe ED has a nozzle *f* pierced with small holes, pointing from a centre in every direction. The keys of the two cocks C and D are united, and the handle *g h* is called the REGULATOR.

Let the regulator be so placed that the steam-cock C is open and the injection-cock D is shut; put water into the boiler A, and make it boil strongly. The steam coming from it will enter the receiver, and gradually warm it, much steam being condensed in producing this effect. When it has been warmed so as to condense no more, the steam proceeds into the rising pipe; the valve G remains shut by its weight; the steam lifts the valve I, and gets into the rising pipe, and gradually warms it. When the workman feels this to be the case, or hears the rattling of the valve I, he immediately turns the steam-cock so as to shut it, the injection-cock still remaining shut (at least we may suppose this for the present.). The apparatus must now cool, and the steam in the receiver collapses into water. There is nothing now to balance the pressure of the atmosphere; the valve I remains shut by its weight; but the air incumbent on the water in the pit presses up this water through the suction-pipe HG, and causes it to lift the valve G, and flow into the receiver R, and fill it to the top, if not more than 20 or 25 feet above the surface of the pit-water.

The steam-cock is now opened. The steam which during the cooling of the receiver has been accumulating in the boiler, and acquiring a great elasticity by the action of the fire, now rushes in with great violence, and pressing on the surface of the water in the receiver, causes it to shut the valve G and open the valve I by its weight alone, and it now flows into the rising pipe, and would stand on a level if the elasticity of the steam were no more than what would balance the atmospherical pressure. But it is much more than this, and therefore it *presses* the water out of the receiver into the rising pipe, and will cause it to come out at K, if the elasticity of the steam is sufficiently great. In order to ensure this, the boiler has another pipe in its top, covered with a *safety*-valve V, which is kept down by a weight W suspended on a steelyard LM. This weight is so adjusted that its pressure on the safety-valve is somewhat greater than the pressure of a column of water V *k* as high as the point of discharge K. The fire is so regulated that the steam is always issuing a little by the loaded valve V. The workman keeps the steam-valve open till he hears the valve I rattle. This tells him that the water is all forced out of the receiver, and that the steam is now following it. He immediately turns the regulator which shuts the steam-cock, and now, for the first time, opens the injection-cock. The cold water trickles at first through the holes of the nozzle *f*, and falling down through the steam, begins to condense it; and then its elasticity being less than the pressure of the water in the pipe KED*f*, the cold water spouts in all directions through the nozzle, and, quick as thought, produces a complete condensation. The valve G now opens again by the pressure of the atmosphere on the water of the pit, and the receiver is soon filled with cold water. The injection-cock is now shut, and the steam-cock opened, and the whole operation is now repeated; and so on continually.

This is the simple account of the process, and will serve to give the reader an introductory notion of the operation; but a more minute attention must be paid to many particulars before we can see the properties and defects of this ingenious machine.

6. The water is driven along the rising pipe by the elasticity of the steam. This must in the boiler, and every part of the machine, exert a pressure on every square inch of the vessels equal to that of the upright column of water. Suppose the water to be raised 100 feet, about 25 of this may be done in the suction-pipe; that is, the upper part of the receiver may be about 25 feet above the surface of the pit-water. The remaining 75 must be done by forcing, and every square inch of the boiler will be squeezed out by a pressure of more than 30 pounds. This very moderate height, therefore, requires very strong vessels; and the marquis of Worcester was well aware of the danger of their bursting. By consulting the table of the elasticity of steam deduced from our experiments mentioned in the preceding article, we see that this temperature must be at least 265° of Fahrenheit's thermometer. *In this heat soft solder is weak, and spelter solder, or good rivetting, ought only to be used.* Accordingly, in a machine erected by Captain Savary, the workman having loaded the safety-valve a little more than usual to make the engine work more briskly, the boiler burst with a dreadful explosion, and blew up the furnace and adjoining parts of the building as if it had been gunpowder. Mr Savary succeeded pretty well in raising moderate quantities of water to small heights, but could make nothing of deep mines. Many attempts were made, on the marquis's principle, to strengthen the vessels from within by radiated bars and by hoops, but in vain. Very small boilers or evaporators were then tried, kept red-hot,

or nearly so, and supplied with a slender stream of water trickling into them; but this afforded no opportunity of making a collection of steam during the refrigeration of the receiver, so as to have a magazine of steam in readiness for the next forcing operation; and the working of such machines was always an employment of great danger and anxiety.

7. The only situation in which this machine could be employed with perfect safety, and with some effect, was where the whole lift did not exceed 30 or 35 feet. In this case the greatest part of it was performed by the suction-pipe, and a very manageable pressure was sufficient for the rest. Several machines of this kind were erected in England about the beginning of the 18th century. A very large one was erected at a salt-work in the south of France. Here the water was to be raised no more than 18 feet. The receiver was capacious, and it was occasionally supplied with steam from a small salt-pan constructed on purpose with a cover. The entry of the steam into the receiver merely allowed the water to run out of it by a large valve, which was opened by the hand, and the condensation was produced by the help of a small forcing pump, also worked by the hand. In so particular a situation as this, (and many such may occur in the endless variety of human wants,) this is a very powerful engine; and having few moving and rubbing parts, it must be of great durability. This circumstance has occasioned much attention to be given to this first form of the engine, even long after it was supplanted by those of a much better construction. A very ingenious attempt was made very lately to adapt this construction to the uses of the miners. The whole depth of the pit was divided into lifts of 15 feet, in the same manner as is frequently done in pump-machines. In each of these was a suction-pipe, 14 feet long, having above it a small receiver like R, about a foot high, and its capacity somewhat greater than that of the pipe. This receiver had a valve at the head of the

suction-pipe, and another opening outwards into the little cistern, into which the next suction-pipe above dipped to take in water. Each of these receivers sent up a pipe from its top, which all met in the cover of a large vessel above ground, which was of double the capacity of all the receivers and pipes. This vessel was close on all sides. Another vessel of equal capacity was placed immediately above it, with a pipe from its bottom passing through the cover of the lower vessel and reaching near to its bottom. This upper vessel communicates with the boiler, and constitutes the receiver of the steam-engine. The operation is as follows: The lower vessel is full of water. Steam is admitted into the upper vessel, which expels the air by a valve, and fills the vessel. It is then condensed by cold water. The pressure of the atmosphere would cause it to enter by all the suction-pipes of the different lifts, and press on the surface of the water in the lower receiver, and force it into the upper one. But because each suction-pipe dips in a cistern of water, the air presses this water before it, raises it into each of the little receivers which it fills, and allows the spring of the air (which was formerly in them, but which now passes up into the lower receiver) to force the water out of the lower receiver into the upper one. When this has been completed, the steam is again admitted into the upper receiver. This allows the water to run back into the lower receiver, and the air returns into the small receivers in the pit, and allows the water to run out of each into its proper cistern. By this means the water of each pipe has been raised 15 feet. The operation may thus be repeated continually.

The contrivance is ingenious, and similar to those which are to be met with in the hydraulics of Schottus, Sturmius, and other German writers. But the operation must be exceedingly slow; and we imagine that the expence of steam must be great, because it must fill a very large and very cold vessel, which must waste a great portion of it by condensation. We see by some late publications of the very ingenious Mr Blackey, that he is still attempting to maintain the reputation of this machine by some contrivances of this kind; but we imagine that they will be ineffectual, except in some very particular situations.

8. For the very great defect of the machine, even when we can secure it against all risk of bursting, is the prodigious waste of steam, and consequently of fuel. Daily experience shews, that a *small quantity* of cold water is sufficient for producing an almost instantaneous condensation of a great quantity of steam. Therefore when the steam is admitted into the receiver of Savary's engine, and comes into contact with the cold top and cold water, it is condensed with great rapidity; and the water does not *begin* to subside till its surface has become so hot that it condenses no more steam. It may now begin to yield to the pressure of the incumbent steam; but as soon as it descends a little, more of the cold surface of the receiver comes into contact with the steam, and condenses more of it, and the water can descend no farther till this addition of cold surface is heated up to the state of evaporation. This rapid condensation goes on all the while the water is descending. By some experiments made by the writer of this article, it appears that no less than $\frac{11}{12}$ths of the whole steam are uselessly condensed in this manner, and not more than $\frac{1}{12}$th is employed in allowing the water to descend by its own weight; and he has reason to think that the portion thus wasted will be considerably greater, if the steam be employed to *force* the water out of the receiver to any considerable height.

Observe, too, that all this waste must be repeated in every succeeding stroke; for the whole receiver must be cooled again in order to fill itself with water.

9. Many attempts have been made to diminish this waste; but all to little purpose, because the very filling of the receiver with cold water occasions its sides to condense a prodigious quantity of steam in the succeeding stroke. Mr

Blackey has attempted to lessen this by using two receivers. In the first was oil; and into this only the steam was admitted. This oil passed to and fro between the two receivers, and never touched the water except on a small surface.[*] But this hardly produced a sensible diminution of the waste: for it must now be observed, that there is a necessity for the first cylinder's being cooled to a considerable degree below the boiling point; otherwise, though it will condense much steam, and allow the water to rise into the receiver, there will be a great diminution of the height of suction, unless the vessel be much cooled. This appears plainly by inspecting the table of elasticity. Thus, if the vessel be cooled no lower than 180°, we should lose one half of the pressure of the atmosphere; if cooled to 120, we should still lose $\frac{1}{10}$th. The inspection of this table is of great use for understanding and improving this noble machine; and without a constant recollection of the elasticity of steam corresponding to its actual heat, we shall never have a notion of the niceties of its operation.

The reader must now be so well acquainted with what passes in the steam-vessel, and with the exterior results from it, as readily to comprehend the propriety of the changes which we shall now describe as having been made in the construction and principle of the steam-engine.

10. Of all places in England, the tin-mines of Cornwall stood most in need of hydraulic assistance; and Mr Savary was much engaged in projects for draining them by his steam-engine. This made its construction and principles well known among the machinists and engineers of that neighbourhood. Among these were a Mr Newcomen, an iron-monger or blacksmith, and Mr Cawley, a glazier at Dartmouth, in Devonshire, who had dabbled much with this machine. Newcomen was a person of some reading, and

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[*] "In this case the oil would be decomposed by the solvent power of steam." W.

was in particular acquainted with the person, writings, and projects of his countryman Dr Hooke. There are to be found among Hooke's papers, in the possession of the Royal Society, some notes of observations, for the use of Newcomen his countryman, on Papin's boasted method of transmitting to a great distance the action of a mill by means of pipes. Papin's project was to employ the mill to work two air-pumps of great diameter. The cylinders of these pumps were to communicate by means of pipes with equal cylinders furnished with pistons, in the neighbourhood of a distant mine. These pistons were to be connected, by means of levers, with the piston-rods of the mine. Therefore, when the piston of the air-pump at the mill was drawn up by the mill, the corresponding piston at the side of the mine would be pressed down by the atmosphere, and thus would raise the piston-rod in the mine, and draw the water. It would appear from these notes, that Dr Hooke had dissuaded Mr Newcomen from erecting a machine on this principle, of which he had exposed the fallacy in several discourses before the Royal Society. One passage is remarkable. "Could he (meaning Papin) make a speedy vacuum under your second piston, your work is done."

11. It is highly probable that, in the course of this speculation, it occurred to Mr Newcomen that the vacuum he so much wanted might be produced by steam, and that this gave rise to his new principle and construction of the steam-engine. The specific desideratum was in Newcomen's mind; and therefore, when Savary's engine appeared, and became known in his neighbourhood many years after, he would readily catch at the help which it promised.

Savary, however, claims the invention as his own; but Switzer, who was personally acquainted with both, is positive that Newcomen was the inventor. By his principles (as a quaker) being averse from contention, he was contented to share the honour and the profits with Savary, whose acquaintance at court enabled him to procure the patent in 1705, in which all the three were associated.

Posterity has done justice to the modest inventor, and the machine is universally called NEWCOMEN'S ENGINE. Its principle and mode of operation may be clearly conceived as follows.

Let A (fig. 7.) represent a great boiler properly built in a furnace. At a small height above it is a cylinder CBBC of metal, bored very truly and smoothly.* The boiler communicates with this cylinder by means of the throat or steam-pipe NQ. The lower aperture of this pipe is shut by the plate N, which is ground very flat, so as to apply very accurately to the whole circumference of the orifice. This plate is called the regulator or steam-cock, and it turns horizontally round an axis *b a*, which passes through the top of the boiler, and is nicely fitted to the socket, like the key of a cock, by grinding. The upper end of this axis is furnished with a handle *b* T.

A piston P is suspended in this cylinder, and made air-tight by a packing of leather or soft rope, well filled with tallow,† and, for greater security, a quantity of water is kept above the piston. The piston-rod PD is suspended by a chain which is fixed to the upper extremity F of the arched head FD of the great lever, or WORKING BEAM HK, which turns on the gudgeon O. There is a similar arched head EG at the other end of the beam. To its upper extremity E is fixed a chain carrying the pump-rod XL, which raises the water from the mine. The load on this end of the beam is made to exceed considerably the weight of the piston P at the other extremity.

At some small height above the top of the cylinder is a cistern W, called the INJECTION CISTERN. From this descends the INJECTION PIPE ZSR, which enters the cylinder through its bottom, and terminates in a small hole R, or sometimes in a nozzle pierced with many smaller holes diverging from a centre in all directions. This pipe has at S a cock, called the INJECTION COCK, fitted with a handle V.

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* This ought to have been the case, but Mr Watt found them generally very much otherwise.

† "Tallow, was only used to lessen the friction when the packing of the piston was renewed, not to keep the piston tight; for that, the water was depended on." W.

At the opposite side of the cylinder, a little above its bottom, there is a lateral pipe, turning upwards at the extremity, and there covered by a clack-valve *f*, called the SNIFTING-VALVE, which has a little dish round it to hold water for keeping it air-tight.

There proceeds also from the bottom of the cylinder a pipe *d e g h* (passing behind the boiler), of which the lower end is turned upwards, and is covered with a valve *h*. This part is immersed in a cistern of water Y, called the HOT WELL, and the pipe itself is called the EDUCTION PIPE. Lastly, the boiler is furnished with a safety-valve, called the PUPPET-CLACK (which is not represented in this sketch for want of room), in the same manner as Savary's engine. This valve is generally loaded with one or two pounds on the square inch, so that it allows the steam to escape when its elasticity is one-tenth greater than that of common air. Thus all risk of bursting the boiler is avoided, and the pressure outwards is very moderate; so also is the heat. For, by inspecting the table of vaporous elasticity, we see that the heat corresponding to 32 inches of elasticity is only about 216° of Fahrenheit's thermometer.

These are all the essential parts of the engine, and are here drawn in the most simple form, till our knowledge of their particular offices shall shew the propriety of the peculiar forms which are given to them. Let us now see how the machine is put in motion, and what is the nature of its work.

12. The water in the boiler being supposed to be in a state of strong ebullition, and the steam issuing by the safety-valve, let us consider the machine in a state of rest, having both the steam-cock and injection-cock shut. The resting position or attitude of the machine must be such as appears

in this sketch, the pump rods preponderating, and the great piston being drawn up to the top of the cylinder. Now open the steam-cock by turning the handle T of the regulator. The steam from the boiler will immediately rush in, and *being lighter than the air, will take the upper part, and will force the air to issue by the snifting-valve:* But much of it will be condensed by the cold surface of the cylinder and piston, and the water produced from it will trickle down the sides, and run off by the eduction-pipe. This condensation and waste of steam will continue till the whole cylinder and piston are made as hot as boiling water. When this happens, the steam will *also begin to issue through the snifting-valve,* slowly at first, and very cloudy, being mixed with air. The blast at *f* will grow stronger by degrees, and more transparent, having already carried off the greatest part of the common air which filled the cylinder. We supposed that the water was boiling briskly, so that the steam was issuing by the safety-valve which is in the top of the boiler, and through every crevice. The opening of the steam-cock puts an end to this at once, and the cold cylinder abstracts the steam from the boiler with *great* rapidity. We may here mention an accident of which we were witnesses, which also shows the immense rapidity of the condensation. The boiler was in a frail shed at the side of the engine-house; a shoot of snow from the top of the house fell down and broke through the roof of the shed, and was scattered over the head of the boiler, which was of an oblong or oval shape. In an instant the sides of it were squeezed together by the pressure of the atmosphere.

When the manager of the engine perceives that not only the blast at the snifting-valve is strong and steady, but that the boiler is now fully supplied with steam of a proper strength, appearing by the renewal of the discharge at the safety-valve, he shuts the steam-cock, and opens the injection-cock S by turning its handle V. The pressure of the column of water in the injection-pipe ZS immediately forces some water through the spout R. This coming in contact with the pure vapour which now fills the cylinder, condenses it, and thus makes a partial void, into which the more distant steam immediately expands, *and is also condensed.* The water spouts rapidly through the hole R by the joint action of the column ZS, and the pressure of the atmosphere; at the same time the snifting valve *f*, and the eduction-valve *h*, are shut by the unbalanced pressure of the atmosphere. The velocity of the injection water must therefore rapidly increase, and the jet will dash (if single) against the bottom of the piston, and be scattered through the whole capacity of the cylinder. In a very short space of time, therefore, the condensation of the steam becomes *general*, and the elasticity of what remains is *greatly lessened, probably to only* $\frac{1}{3}$, *or* $\frac{1}{4}$ *of that of the atmosphere. Meanwhile the whole pressure of the atmosphere continues to act upon the upper side of the piston, and not being counterbalanced by that of the weak steam which now fills the cylinder; if the load on the outer end of the working beam is inferior to the difference of these pressures*, it must yield to it. The piston P must descend, and the pump piston L must ascend, bringing along with it the water of the mine, and the motion must continue till the great piston reaches the bottom of the cylinder; for it is not like the motion which would take place in a cylinder of air rarefied to the same degree. In this last case, the impelling force would be continually diminished, because the capacity of the cylinder is diminished by the descent of the piston, and the air in it is continually becoming more dense and elastic. The piston would stop at a certain height, where the elasticity of the included air, together with the load at E, would balance the atmospherical pressure on the piston. But when the contents of the cylinder are pure vapour, and the continued stream of injected cold water keeps down its temperature to the same pitch as at the beginning, the elasticity of the remaining steam can never increase by the descent of the piston, nor exceed what corresponds to this temperature. The impelling or accelerating force therefore remains the same, and the descent of the piston will be uniformly accelerated, if there is not an increase of resistance arising from the nature of the work performed by the other end of the beam. This circumstance will come under consideration afterwards, and we need not attend to it at present. It is enough for our present purpose to see that if the cylinder has been completely purged of common air before the steam-cock was shut, and if none has entered since, the piston will descend to the very bottom of the cylinder. And this may be frequently observed in a good steam-engine where every part is air-tight. It sometimes happens, by the pit-pump drawing air, or some part of the communication between the two strains giving way, that the piston comes down with such violence as to knock out the bottom of the cylinder with the blow.

13. The only observation which remains to be made on the motion of the piston in descending is, that it does not begin at the instant the injection is made. The piston was kept at the top by the preponderancy of the outer end of the working beam, and it must remain there till the difference between the elasticity of the steam below it and the pressure of the atmosphere exceeds this preponderancy. There must, therefore, be a small space of time between the beginning of the condensation and the beginning of the motion. This is very small, not exceeding the third or the fourth part of a second; but it may be very distinctly observed by an attentive spectator. He will see, that the instant the injection-cock is opened, the cylinder will sensibly rise upwards a little by the pressure of the air on its bottom. Its whole weight is not nearly equal to this pressure; and instead of its being necessary to *support* it by a strong floor, we must *keep it down* by strong beams loaded by heavy walls. It is usual to frame these beams into the posts which carry the axis of the working-beam, and they are therefore loaded with the whole strain of the machine. This rising of the cylinder shows the instantaneous commence-

ment of the condensation; and it is not till *after* this has been distinctly observed that the piston is seen to start, and begin to descend.

When the manager sees the piston as low as he thinks proper, he shuts the injection-cock, and opens the steam-cock. The steam has been accumulating above the water in the boiler during the whole time of the piston's descent, and is now the puppet-clack. The moment, therefore, that the steam-cock is opened, it rushes violently into the cylinder, having an elasticity greater than that of the air. It therefore immediately blows open the snifting-valve, and allows (at least) the water which had come in by the former injection, and what arose from the condensed steam, to descend by its own weight through the eduction pipe *d e g h*, to open the valve *h*, and to run out into the hot well. *A portion of the steam is necessarily condensed in heating the surface of the water in the cylinder, the sides of that vessel and the lower surface of the piston, and although these are already very warm, yet the quantity of steam condensed is considerable; generally much more than what would be necessary to fill a cylinder which was already of the heat of boiling water.*

This first puff of the entering steam is of great service; it drives out of the cylinder the vapour which it finds there. This is seldom pure watery vapour: all water contains a quantity of air in a state of chemical union. The union is but feeble, and a boiling heat is sufficient for disengaging the greatest part of it by increasing its elasticity. It may also be disengaged by simply removing the external pressure of the atmosphere. This is clearly seen when we expose a glass of water in an exhausted receiver. Therefore the small space below the piston contains watery vapour mixed with all the air which had been disengaged from the water in the boiler by ebullition, and all that was separated from the injection water by the diminution of external pressures. All this is blown out of the cylinder by the first puff of steam. We may observe in this place, that waters differ exceedingly in the quantity of air which they hold in a state of solution. All spring water contains much of it: and water newly brought up from deep mines contains a great deal more, because the solution was aided in these situations by great pressure. Such waters sparkle when poured into a glass. It is therefore of consequence to the good performance of a steam-engine, to use water containing little air, both in the boiler and in the injection-cistern. The water of running brooks is preferable to all others; and the freer it is from any saline impregnation, it generally contains less air. *The air collected below the piston diminishes the accelerating force, and the saline matters contained in such waters are also extremely hurtful, by encrusting the boilers, and rendering them less pervious to heat. It is therefore adviseable to keep water so impregnated, in a large shallow pond for some time before it is used; or rather to cool the water which has been used in such a pond, and to use it in place of fresh pit-water, as the heat and exhaustion it has undergone cause it to part with the air it contained, and to deposit the saline or earthy matters.*

Let us now consider the state of the piston. It is evident that it will start or begin to rise the moment the steam-cock is opened; for at that instant the excess of atmospherical pressure, by which it was kept down in opposition to the preponderancy of the outer end of the beam, is diminished. The piston is therefore pulled upwards, and it will rise even although the steam which is admitted be not so elastic as common air. Suppose the mercury in the barometer to stand at 30 inches, and that the preponderancy at the outer end of the beam is th of the pressure of the air on the piston, the piston will not rise if the elasticity of the steam is not equal to $30-\frac{30}{9}$, that is, to 26.7 inches nearly; but if it is just this quantity, the piston will rise as fast as this steam can be supplied through the steam-pipe, and the velocity of its ascent depends entirely on the velocity of this supply. This observation is of great importance; and it does not seem to have occurred to the mathematicians, who have paid most attention to the mechanism of the motion of this engine. In the mean time, we may clearly see that the entry of the steam depends chiefly on the counter weight at E; for suppose there was none, steam no stronger than air would not enter the cylinder at all; and if the steam be stronger, it will enter only by the excess of its strength. Writers on the steam-engine (and even some of great reputation) familiarly speak of the steam giving the piston a push: But this is scarcely possible. During the rise of the piston the snifting-valve is never observed to blow; and we have not heard any well-attested accounts of the piston-chains ever being slackened by the upward pressure of the steam, even at the very beginning of the stroke. During the rising of the piston, the steam is (according to the common conception and manner of speaking) *sucked in*, in the same way that air is sucked into a common syringe or pump when we draw up the piston; for in the steam-engine the piston is really drawn up by the counter weight. But it is still more sucked in, and requires a more copious supply, for another reason. As the piston descended only in consequence of the inside of the cylinder's being sufficiently cooled to condense the steam, this cooled surface must again be presented to the steam during the rise of the piston, and must condense steam a second time. The piston cannot rise another inch till the part of the cylinder which the piston has already quitted, has been warmed up to the boiling point, and steam must be expended in this warming. The inner surface of the cylinder is not only of the heat of boiling water while the piston rises, but is also perfectly dry; for the film of water left on it by the ascending piston must be completely evaporated, otherwise it will be condensing steam. *The quantity thus wasted is, as we have said, considerable; it varies in different engines according to their load, and other circumstances, from three quarters of one fill to two fills of the cylinder, as we have* *learnt from Mr Watt's experiments. The experiments which Dr Desaguliers relates as made by Mr Beighton upon the consumption of steam by a certain engine, were in themselves erroneous, and the Doctor's calculations founded upon them were still more so. From them he made the deduction, that steam was* 14,000 *times rarer than water; but Mr Watt, whose experiments have been fully verified by long practice on a great scale, makes it, when under the pressure of the atmosphere and of the heat of* 213°, *onl from* 1800 *to* 2000 *times less heavy than the same bulk of water.*

18. The moving force during the ascent of the piston must be considered as resulting chiefly, if not solely, from the preponderating weight of the pit piston-rods. The office of this is to return the steam-piston to the top of the cylinder, where it may again be pressed down by the air, and make another working stroke by raising the pump-rods. But the counter-weight at E has another service to perform in this use of the engine, namely, to return the pump-pistons into their places at the bottom of their respective working barrels, in order that they may make a working-stroke. This requires force independent of the friction and inertia of the moving parts; for each piston must be pushed down through the water in the barrel, which must rise through the piston with a velocity whose proportion to the velocity of the piston is the same with that of the area of the piston to the area of the perforation through which the water rises through the piston. It is enough at present to mention this in general terms: we shall consider it more particularly afterwards, when we come to calculate the performance of the engine, and to deduce from our acquired knowledge maxims of construction and improvement.

19. From this general consideration of the ascent of the piston, we may see that the motion differs greatly from the descent. It can hardly be supposed to accelerate, even if the steam in the cylinder were in a moment annihilated. For the resistance to the descent of the piston is the same

with the weight of the column of water, which would cause it to flow through the *opening* of the pump-piston with the velocity with which it really rises through it, and must therefore increase as the square of that velocity increases; that is, as the square of the velocity of the piston increases. Independent of friction, therefore, the velocity of descent through the water must soon become a maximum, and the motion become uniform. We shall see by and by, that in such a pump as is generally used this will happen in less than the 10th part of a second. The friction of the pump will diminish this velocity a little, and retard the time of its attaining uniformity. But, on the other hand, the supply of steam which is necessary for this motion, being susceptible of no acceleration from its previous motion, and depending entirely on the briskness of the ebullition, an almost instantaneous stop is put to acceleration.

Accordingly, any person who observes with attention the working of a steam-engine, will see that the rise of the piston and descent of the pump-rods is extremely uniform, whereas the working-stroke is very sensibly accelerated.

90. Before quitting this part of the subject, and lest it should afterwards escape our recollection, we may observe, that the counter-weight is different during the two motions of the pump-rods. *The machine, when making a working-stroke,* is lifting not only the column of water in the pump, but the absolute weight of the *pump-pistons and pump-rods* also: but while the pump-rods are descending, there is a diminution of the counter-weight by the whole weight lost by the immersion of the rod in water. The wooden rods which are generally used, soaked in water, and joined by iron straps, are heavier, and but a little heavier, than water, and they are generally about one-third of the bulk of the water in the pumps.

These two motions complete the period of the operation; and the whole may be repeated by shutting the steam-cock and opening the injection-cock whenever the piston has attained the proper height. We have been very minute in our attention to the different circumstances, that the reader may have a distinct notion of the state of the moving forces in every period of the operation. It is by no means sufficient that we know in general that the injection of cold water makes a void which allows the air to press down the piston, and that the readmission of the steam allows the piston to rise again. This slovenly way of viewing it has long prevented even the philosopher from seeing the defects of the construction, and the methods of removing them.

21. We now see the great difference between Savary's and Newcomen's engine in respect of principle. Savary's was really an engine which raised water *partly by the force of steam, and partly by the pressure of the atmosphere;* but Newcomen's raises water entirely by the pressure of the atmosphere, and steam is employed merely as the most expeditious method of producing a void, into which the atmospherical pressure may impel the *first mover* of his machine. The elasticity of the steam is not the first mover.

22. We see also the great superiority of this new machine. We have no need of steam of great and dangerous elasticity; and we operate by means of very moderate heats, and consequently with much smaller quantities of fuel; *and the boundaries to the power of this machine are not the strength which we can give to boilers and cylinders to resist internal pressure, but the dimensions to which we may find it practicable or expedient to make them, and other parts of the machine, such as the working-beam, or great lever, and its appendages.* And, lastly, this form of the machine renders it applicable to almost every mechanical purpose; because a skilful mechanic can readily find a method of converting the reciprocating motion of the working-beam into a motion of any kind which may suit his purpose. Savary's engine could hardly admit of such an immediate application, and seems almost restricted to raising water.

23. Inventions improve by degrees. This engine was first offered to the public in 1705. But many difficulties occurred in the execution, which were removed one by one; and it was not till 1712 that the engine seemed to give confidence in its efficacy. The most exact and unremitting attention of the manager was required to the precise moment of opening and shutting the cocks; and neglect might frequently be ruinous, by beating out the bottom of the cylinder, or allowing the piston to be wholly drawn out of it. Stops were contrived to prevent both of these accidents; then strings were used to connect the handles of the cocks with the beam, so that they should be turned whenever it was in certain positions.

24. These strings were gradually changed and improved into detents and catches of different shapes; at last, in 1717, Mr Beighton, a very ingenious and well-informed artist, simplified the whole of these subordinate movements, and brought the machine into the form in which it has continued, without the smallest material change, to the present day. We shall now describe one of these improved engines, copying nearly the drawings and description given by Bossut in his *Hydrodynamique;* these being by far the most accurate and perspicuous of any that have been published.

25. Fig. 8. No. 1. Plate II. is a vertical section of the boiler, cylinder, and all the parts necessary for turning the cocks. Fig. 8. No. 2. is a plan of the apparatus for opening and shutting the steam-regulator; and the same pieces of both are marked with the same letters of reference.

The rod X of the piston P is suspended from the arch of the working-beam, as was represented in the preceding sketch (fig. 7.). An upright bar of timber FG is also seen hanging by a chain. This is suspended from a concentric arch of the beam, as may be seen also in the sketch at φ δ. The bar is called the *plug-beam;* and it must rise and fall with the piston, but with a slower motion. The use of this plug-beam is to give motion to the different pieces which turn the cocks.

The steam-pipe K is of one piece with the bottom of the cylinder, and rises within it three or four inches, to prevent any of the cold injection water from falling into the boiler. The lower extremity Z of the steam-pipe penetrates the head of the boiler, projecting a little way. A flat plate of brass, in shape resembling a racket or battledore, called the *regulator*, applies itself exactly to the whole circumference of the steam-pipe, and completely excludes the steam from the cylinder. Being moveable round an upright axis, which is represented at the side of the steam-pipe in the profile, it may be turned aside by the handle *i*, No. 1. and 2. The profile shews in the section of this plate a protuberance in the middle. This rests on a strong flat spring, which is fixed below it athwart the mouth of the steam-pipe. This spring presses it strongly towards the steam-pipe, causing it to apply very close; and this knob slides along the spring, while the regulator turns to the right or left.

We have said that the injection-water is furnished from a cistern placed above the cylinder. When the cistern cannot be supplied by pipes from some more elevated source, its water is raised by the machine itself. A small lifting pump *i k* (fig. 7. Plate I.), called the *jack-head* or *jaquette*, is worked by a rod *i* ι, suspended from a concentric arch *i* γ near the outer end of the working-beam. This forces a small portion of the pit-water along the rising pipe *i* L′ into the injection cistern.

In figure 8. No. 1. the letters QM S′ represent the pipe which brings down the water from the injection cistern. This pipe has a cock at R to open or shut the passage of this water. It spouts through the jet S′, and dashing against the bottom of the piston, it is dispersed into drops, and scattered through the whole capacity of the cylinder, so as to produce a rapid condensation of the steam.

An upright post A supports one end B of a horizontal iron axis BC. The end of C is supported by a similar post, shewn only in the plan No. 2., that the pieces connected with this axis may not be hid by it. A kind of stirrup *a b c d* hangs from this axis, supported by the hooks *a* and *d*. This stirrup is crossed near the bottom by a round bolt or bar *e*, which passes through the eyes or rings that are at the ends of the horizontal fork *h f g*, whose long tail *h* is double, receiving between its branches the handle *i* of the regulator. It is plain from this construction, that when the stirrup is made to vibrate round the horizontal axis BC, on which it hangs freely by its hooks, the bolt *e* must pull or push the long fork *h f g* backwards and forwards horizontally, and by so doing will move the regulator round its axis by means of the handle *i*. Both the tail of the fork and the handle of the regulator are pierced with several holes, and a pin is put through them which unites them by a joint. The motion of the handle may be increased or diminished by choosing for the joint a hole near to the axis or remote from it; and the exact position at which the regulator is to stop on both sides is determined by pins stuck in a horizontal bar on which the end of the handle is made to rest.

This alternate motion of the regulator to the right and left is produced as follows: There is fixed to the axis BC a piece of iron *o k l*, called the Y, on account of its resemblance to that letter of the alphabet inverted. The stalk *o* carries a heavy lump *p*, called the loggerhead, of lead or iron; and a long leather strap *q p r* is fastened to *p* by the middle, and the two ends are fastened to the beam above it, in such a manner that the lump may be alternately catched and held up to the right and left of the perpendicular. By adjusting the length of the two parts of the strap, the Y may be stopped in any desired position. The two claws *k* and *l* spread out from each other, and from the line of the stalk, and they are of such length as to reach the horizontal bolt *e*, which crosses the stirrup below, but not to reach the bottom of the fork *h f g*.

Now suppose the stirrup hanging perpendicularly, and the stalk of the Y also held perpendicular; carry it a little outward from the cylinder, and then let it go. It will tumble farther out by its weight, without affecting the stirrup till the claw *l* strikes on the horizontal bolt *e*, and then it pushes the stirrup and the fork towards the cylinder, and opens the regulator. It sets it in motion with a smart jerk, which is an effectual way of overcoming the cohesion and friction of the regulator with the mouth of the steam-pipe. This push is adjusted to a proper length by the strap *q p*, which stops the Y when it has gone far enough. If we now take hold of the stalk of the Y, and move it up to the perpendicular, the width between its claws is such as to permit this motion, and something more, without affecting the stirrup. But when pushed still nearer to the cylinder, it tumbles towards it by its own weight, and then the claw *k* strikes the bolt *e*, and drives the stirrup and fork in the opposite direction, till the lump *p* is catched by the strap *r p*, now stretched to its full length, while *q p* hangs slack. Thus by the motion of the Y the regulator is opened and shut. Let us now see how the motion of the Y is produced by the machine itself. To the horizontal axis BC are attached two spanners or handles *m* and *n*. The spanner *m* passes through a long slit in the plug-beam, and is at liberty to move upwards or downwards by its motion round the axis BC. A pin *x* which goes through the plug-beam catches hold of *m* when the beam rises along with the piston; and the pin is so placed, that when the beam is within an inch or two of its highest rise, the pin has lifted *m* and thrown the stalk of the Y past the perpendicular. It therefore tumbles over with great force, and gives a smart blow to the fork, and immediately shuts the regulator. By this motion the spanner *m* is removed out of the neighbourhood of the plug-beam. But the spanner *n*, moving along with it in the same direction, now comes into the way of the pins of the plug-beam. Therefore, when the piston descends again by the condensation of the steam in the cylinder, a pin marked & in the side of the plug-beam, catches hold of the tail of the spanner *n*, and by pressing it down raises the lump on the stalk of the Y till it passes the perpendicular, and it then falls down, outwards from the cylinder, and the claw *l* again drives the fork in the direction *h i*, and opens the steam-valve. This opening and shutting of the steam-valve is executed in the precise moment that is proper, by placing the pins *x* and & at a proper height of the plug-beam. For this reason, it is pierced through with a great number of holes, that the places of these pins may be varied at pleasure. This, and a proper curvature of the spanners *m* and *n*, make the adjustment as nice as we please.

The injection-cock R is managed in a similar manner. On its key may be observed a forked arm *s t*, like a crab's claw; at a little distance above it is the gudgeon or axis *u* of a piece *y u z*, called the hammer or the F, from its resemblance to that letter. It has a lump of metal *y* at one end, and a spear *u s* projects from its middle, and passes between the claws *s* and *t* of the arm of the injection-cock. The hammer *y* is held up by a notch in the under side of a wooden lever DE, moveable round the centre D, and supported at a proper height by a string *r* E, made fast to the joist above it.

Suppose the injection-cock shut, and the hammer in the position represented in the figure. A pin *β* of the plug-frame rises along with the piston, and catching hold of the detent DE, raises it, and disengages the hammer *y* from its notch. This immediately falls down, and strikes a board L put in the way to stop it. The spear *u s* takes hold of the claw *t*, forces it aside, and opens the injection-cock. The piston immediately descends, and along with it the plug-frame. During its descent the pin *β* meets with the tail *u z* of the hammer, which is now raised considerably above the level, and brings it down along with it, raising the lump *y*, and gradually shutting the injection-cock, because the spear takes hold of the claw *s* of its arm. When the beam has come to its lowest situation, the hammer is again engaged in the notch of the detent DE, and supported by it till the piston again reaches the top of the cylinder.

In this manner the motions of the injection-cock are also adjusted to the precise moment that is proper for them. The different pins are so placed in the plug-frame, that the steam-cock may be completely shut before the injection-cock is opened. The inherent motion of the machine will give a small addition to the ascent of the piston without expending steam all the while; and by leaving the steam rather less elastic than before, the subsequent descent of the piston is promoted. There was considerable propriety in the gradual shutting of the injection-cock. For after the first dash of the cold water against the bottom of the piston, the condensation is nearly complete, and very little more water is needed; but a continual accession of some is absolutely necessary for completing the condensation, as the capacity of the cylinder diminishes, and the water warms which is already injected.

In this manner the motion of the machine will be repeated as long as there is a supply of steam from the boiler, and of water from the injection cistern, and a discharge procured for what has been injected. We proceed to consider how far these conditions also are provided by the machine itself.

The injection cistern is supplied with water by the jack-head pump, as we have already observed. From this source all the parts of the machine receive their respective supplies. In the first place, a small branch 13, is taken off from the injection-pipe immediately below the cistern, and conducted to the top of the cylinder, where it is furnished with a cock. The spout is so adjusted, that no more runs from it than what will keep a constant supply of water above the piston to keep it tight. Every time the piston comes to the top of the cylinder, it brings this water along with it, and the surplus of its evaporation and leakage runs off by a waste-pipe 14. This water necessarily becomes *very* hot, and it was thought proper to employ its overplus for supplying the waste of the boiler. This was accordingly practised for some time. But Mr Beighton improved this economical thought, by supplying the boiler from the eduction-pipe, 2, 2, the water of which must be still hotter than that above the piston. This contrivance required attention to many circumstances, which the reader will understand by considering the profile. The eduction-pipe comes out of the bottom of the cylinder at 1, with a perpendicular part, which bends sidewise below, and turns up the deep cup 5, holding a metal valve nicely fitted to it by grinding, like the key of a cock. To secure its being always air-tight, a slender stream of water trickles into this cup from a branch 6 of the waste-pipe from the top of the cylinder. The eduction-pipe branches off at 2, and goes down to the hot well, where it turns up, and is covered with a valve. In the section may be observed an upright pipe 4, 4, which goes through the head of the boiler, and reaches to within a few inches of its bottom. This pipe is called the *feeder*, and rises about three or four feet above the boiler. It is open at both ends, and has a branch 3, communicating with the bottom of the cup 5, immediately above the metal valve, and also a few inches below the level of the entry 2 of the eduction-pipe. This communicating branch has a cock by which its passage may be diminished at pleasure. Now suppose the steam in the boiler to be very strong, it will cause the boiling water to rise in the feeding-pipe above 3, and coming along this branch, to rise also in the cup 5, and run over. But the height of this cup above the surface of the water in the boiler is such, that the steam is never strong enough to produce this effect. Therefore, on the contrary, any water that may be in the cup 5 will run off by the branch 3, and go down into the boiler by the feeding pipe.

26. These things being understood, let us suppose a quantity of injected water lying at the bottom of the cylinder. It will run into the eduction-pipe, fill the crooked branch, and open the valve in the bottom of the cup; (its weight being supported by a wire hanging from a slender spring,) and it will fill the cup to the level of the entry 2 of the eduction-pipe, and will then flow along 3, and supply the boiler by the feeder 4, 4. What more water runs in will now go along the eduction-pipe 2, 2, to the hot well. By properly adjusting the cock on the branch 3, 3, the boiler may be supplied as fast as the waste in steam requires. This is a most ingenious contrivance, and does great honour to Mr Beighton. It is not, however, of *very great* importance. The small quantity which the boiler requires may be immediately taken even from a cold cistern, without *much* diminishing the production of steam; for the quantity of heat necessary for raising the sensible heat of cold water to the boiling temperature is small, when compared with the quantity of heat which must then be combined with it in order to convert the water into steam. For the heat expended in boiling off a cubic foot of water is about six times as much as would bring it to a boiling heat from the temperature of 55°. It has, however, the advantage of being purged of air; and when an engine must derive all its supplies from pit water, the water from the eduction-pipe is vastly preferable to that from the top of the cylinder.

We may here observe, that many writers (among them the Abbé Bossut), in their descriptions of the steam-engine, have drawn the branch of communication 3, from the feeding-pipe to a part of the crooked pipe 1, lying below the valve in the cup 5. But this is quite erroneous; for, in this case, when the injection is made into the cylinder, and a vacuum produced, the water from the boiler would immediately rush up through the pipes 4, 3, and spout up into the cylinder: so would the external air coming in at the top of the feeder.

27. This contrivance has also enabled us to form some judgment of the internal state of the engine during the performance. Mr Beighton paid a minute attention to the situation of the water in the feeders and eduction-pipe of an engine, which seems to have been one of the best which *had then* been erected. It was lifting a column of water whose weight was four-sevenths of the pressure of the air on its piston, and made 16 strokes, of six feet each, in a minute. This is acknowledged by all to be a very great performance of an engine of this form.* He concluded that the elasticity of the steam in the cylinder was never more than one-tenth greater or less than the elasticity of the air. The water in the feeder never rose more than three feet and a half above the surface of the boiling water, even though it was now lighter by one-twentieth than cold water. The eduction-pipe was only four feet and a half long (vertically), and yet it always discharged the injection water completely, and allowed some to pass into the feeder. This could not be if the steam was much more than one-tenth weaker than air. By grasping this pipe in his hand during the rise of the piston, he could guess very well whereabouts the surface of the hot water in it rested during the motion, and he never found it supported so high as four feet. Therefore the steam in the cylinder had at least eight-ninths of the elasticity of the air. Mr Buat, in his examination of an engine which is erected at Montrelaix, in France, by an English engineer, and has always been considered as the pattern in that country, finds it necessary to suppose a

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* "In so far as regards the load; but were we furnished with facts to judge of its consumption of steam compared with the work done, we should undoubtedly find it very defective when compared with others more lightly loaded, and in other respects equally well constructed." W.

much greater variation in the strength of the steam, and says, that it must have been one-fifth stronger and one-fifth weaker than common air. But this engine has not been nearly so perfect. Its lift was not more than one-half of the pressure of the atmosphere, and it made but nine strokes in a minute.—At W is a valve covering the mouth of a small pipe, and surrounded with a cup containing water to keep it air-tight. This allows the air to escape which had been extricated from the water of last injection. It is driven out by the first strong puff of steam which is admitted into the cylinder, and makes a noise in its exit. The valve is therefore called the snifting-valve.

To finish our description, we observe, that besides the safety-valve 9 (called the PUPPET-CLACK), which is loaded with about three pounds on the square inch (though the engine will work very well with a load of one or two pounds), there is another DISCHARGER 10, having a clack at its extremity supported by a cord. Its use is to discharge the steam without doors, when the machine gives over working. There is also a pipe SI near the bottom of the boiler, by which it may be emptied when it needs repairs or cleansing.

There are two small pipes 11, and 12, with cocks, called GAGE-PIPES. The first descends to within two inches of the surface of the water in the boiler, and the second goes about two inches below that surface. If both cocks emit steam, the water is too low, and requires a recruit. If neither give steam, it is too high, and there is not sufficient room above it for a collection of steam. Lastly, there is a filling pipe Q, by which the boiler may be filled when the machine is to be set to work.

28. The engine has continued in this form for many years. The only remarkable change introduced has been the manner of placing the boiler. It is no longer placed below the cylinder, but at one side, and the steam is introduced by a pipe from the top of the boiler into a flat box immediately

below the cylinder. The use of this box is merely to lodge the regulator, and give room for its motions. This has been a very considerable improvement. It has greatly reduced the height of the building. This was formerly a tower. The wall which supported the beam could hardly be built with sufficient strength for withstanding the violent shocks which were repeated without ceasing; and the buildings seldom lasted more than a very few years. But the boiler is now set up in an adjoining shed, and the gudgeons of the main beam rest on the top of upright posts, which are framed into the joists which support the cylinder. Thus the whole moving parts of the machine are contained in one compact frame of carpentry, and have little or no connection with the slight walls of the building, which is merely a case to hold the machine, and protect it from the weather.

29. It is now time to enquire what is to be expected from this machine, and to ascertain the most advantageous proportion between the moving power and the load that is to be laid on the machine.

It may be considered as a great pulley, and is indeed sometimes so constructed, the arches at the ends of the working-beam being completed to a circle. It must be unequally loaded that it may move. It is loaded, during the working-stroke, by the pressure of the atmosphere on the side *of the cylinder*, and by the column of water to be raised and the pump-gear on the pump side.—During the returning stroke it is loaded, on the side *of the cylinder*, by a small part of the atmospheric pressure, and on the pump side by the pump-gear acting as a counter weight. The load during the working stroke must therefore consist of the column of water to be raised and this counter-weight. The performance of the machine is to be measured only by the quantity of water raised in a given time to a given height. It varies, therefore, in the joint proportion of the weight of the column of water in the pumps, and the number of strokes made by the machine in a minute. Each stroke consists of two parts, which we have called the working and the returning-stroke. It does not, therefore, depend simply on the velocity of the working-stroke and the quantity of water raised by it. If this were all that is to be attended to, we know that the weight of the column of water should be nearly $\frac{4}{9}$ths of the pressure of the atmosphere, this being the proportion which gives the maximum in the common pulley. But the time of the returning-stroke is a necessary part of the whole time elapsed, and therefore the velocity of the returning-stroke equally merits attention. This is regulated by the counter-weight. The number of strokes per minute does not give an immediate proof of the goodness of the engine. A small load of water and a great counter-weight will ensure this, because these conditions will produce a brisk motion in both directions.—The proper adjustment of the pressure of the atmosphere on the piston, the column of water to be raised, and the counter-weight, is a problem of very great difficulty; and mathematicians have not turned much of their attention to the subject, although it is certainly *a very* interesting question.

30. Mr Bossut has solved it very shortly and simply, upon this supposition, that the working and returning stroke should be made in equal times.* This, indeed, is generally aimed at in the erection of these machines, and they are not reckoned to be well arranged if it be otherwise. We doubt of the propriety of the maxim. Supposing, however, this

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* "I feel myself perfectly at a loss how to correct articles 30, 31, 32, 33, 34, 35, 36, and 37, as they involve algebraic calculations which seem in a great degree unnecessary, as is acknowledged in art. 43, being founded on principles not sufficiently known to be subjected to rule ; but as they contain much ingenious reasoning, and may lead to the formation of more correct formulæ, I have not considered myself at liberty to alter them, but have added notes, correcting some of the facts and some part of the reasoning." W.

condition for the present, we may compute the loadings of the two ends of the beam as follows: Let a be the length of the inner arm of the working-beam, or that by which the great piston is supported. Let b be the outer arm carrying the pump-rods, and let W be a weight equivalent to all the load which is laid on the machine. Let c^2 be the area of the piston; let H be the height of a column of water, having c^2 for its base, and being equal in weight to the pressure exerted by the steam on the under side of the piston; and let h be the pressure of the atmosphere on the same area, or the height of a column of water of equal weight. It is evident that both strokes will be performed in equal times, if $h\,c^2\,a - W\,b$ be equal to $(h - H)\,c^2\,a + W\,b$. The first of these quantities is the energy of the machine during the working-stroke, and the second expresses the similar energy during the returning-stroke. This equation give us $W = \frac{2\,h\,c^2\,a - H\,c^2\,a}{2\,b} = \frac{(2h - H)\,c^2\,a}{2\,b}$. If we suppose the arms of the lever equal and $H = h$, we have $W = c^2\frac{h}{2}$; that is, the whole weight of the outer end of the beam should be half the pressure of the air on the great piston. This is nearly the usual practice, and the engineers express it by saying, that the engine is loaded with seven or eight pounds on the square inch. This has been found to be nearly the most advantageous load.

31. This way of expressing the matter would do well enough, if the maxim were not founded on erroneous notions which hinder us from seeing the state of the machine, and the circumstances on which its improvement depends. The piston bears a pressure of 15 pounds, it is said, on the square inch, if the vacuum below it be perfect. *But to produce a perfect vacuum, it will appear by the tables of elasticities we have given (see* STEAM*, art. 22, 23), that the cylinder and its contents must be cooled down below 32° each stroke, and*

were this practicable, the consumption of steam in re-heating the cylinder would be enormous; it has therefore been found advisable in practice to throw in no more injection water than will produce a vacuum sufficient to enable the engine to raise a load of from six to seven pounds on the square inch, besides all friction, and other ineffective burthens; and it has been found that, under the lesser load above stated, the engines consumed less steam in proportion to the work done, than under the greater one.

32. This equation by Mr Bossut is moreover essentially faulty in another respect. The W in the first member is not the same with the W in the second. In the first, it is the column of water to be raised, together with the counter-weight. In the second, it is the counter-weight only. Nor is the quantity H the same in both cases, as is most evident. The proper equation for ensuring the equal duration of the two strokes may be had in the following manner: Let it be determined by experiment what portion of the atmospheric pressure is exerted on the great piston during its descent. This depends on the remaining elasticity of the steam. Suppose it $\frac{8}{10}$ths: this we may express by $a\ h$, a being $=\frac{8}{10}$ths. Let it also be determined by experiment what portion of the atmospheric pressure on the piston remains unbalanced by the steam below it during its ascent. Suppose this $\frac{2}{10}$th, we may express this by $b\ h$. Then let W be the weight of the column of water to be raised, and c the counter-weight. Then, if the arms of the beam are equal, we have the energy during the working-stroke $= a\ h - \mathrm{W} - c$, and during the returning-stroke it is $= c - b\ h$. Therefore $c - b\ h = a\ h - \mathrm{W} - c$; and $c = \frac{h\,(a+b) - \mathrm{W}}{2}$; which, on the above supposition of the values of a and b, gives us $c = \frac{h - \mathrm{W}}{2}$. We shall make some use of this equation afterwards; but it affords us no inform-

The fourth term of this analogy is the space required.

Let t be the whole time of the descent in seconds. Then $1^2 : t^2 = \frac{g(p-m)}{p} : \frac{t^2 g(p-m)}{p}$. This last term is the whole descent or length of the stroke accomplished in the time t.

The weight of the column of water, which has now got above the piston, *or bucket*, is w, $= L-p$. This must be lifted in the next working-stroke through the space $\frac{t^2 g(p-m)}{p}$. Therefore the performance of the engine must be

$$\frac{t^2 g(p-m)(L-p)}{p}.$$

That this may be the greatest possible, we must consider p as the variable quantity, and make the fluxion of the fraction $\frac{\overline{p-m} \times \overline{L-p}}{p} = o$.

This will be found to give us $p = \sqrt{Lm}$; that is, the counter-weight or preponderancy of the outer end of the beam is $= \sqrt{Lm}$.

This gives us a method of determining m experimentally. We can discover by actual measurement the quantity L in any engine, it being equal to the unbalanced weights on the beam and the weight of the water in the pumps. Then $m = \frac{p^2}{L}$.

Also we have the weight of the column of water $= L-p$, $= L-\sqrt{Lm}$.

When, therefore, we have determined the load which is to be on the outer end of the beam during the working-stroke, it must be distributed into two parts, which have the proportion of $\sqrt{Lm}$ to $L-\sqrt{Lm}$. The first is the counter-weight, and the second is the weight of the column of water.

If m is a fraction of L, such as an aliquot part of it; that is, if

$$m = \frac{L}{1}, \frac{L}{4}, \frac{L}{9}, \frac{L}{16}, \frac{L}{25}, \&c$$

$$p = \frac{L}{1}, \frac{L}{2}, \frac{L}{3}, \frac{L}{4}, \frac{L}{5}, \&c.$$

The circumstance which is commonly obtruded on us by local considerations is the quantity of water, and the depth from which it is to be raised; that is, w; and it will be convenient to determine every thing in conformity to this.

We saw that $w = L - \sqrt{L\,m}$. This gives us $L = \pm\sqrt{w\,m + \frac{m^2}{4} + \frac{m}{2}} + w$, and the counter-weight $p = \sqrt{w\,m + \frac{m^2}{4} + \frac{m}{2}}$.

35. Having thus ascertained that distribution of the load on the outer end of the beam which produces the greatest effect, we come now to consider what proportion of moving force we must apply, so that it may be employed to the best advantage, or so that any expence of power may produce the greatest performance. It will be so much the greater as the work done is greater, and the power employed is less; and will therefore be properly measured by the quotient of the work done divided by the power employed.

The work immediately done is the lifting up the weight L. In order to accomplish this, we must employ a pressure P, which is greater than L. Let it be $= L + y$; also let s be the length of the stroke.

If the mass L were urged along the space s by the force $L + y$, it would acquire a certain velocity, which we may express by $\sqrt{s}$; but it is impelled only by the force y, the rest of P being employed in balancing L. The velocities which different forces generate by impelling a body along the same space are as the square roots of the forces. Therefore $\sqrt{L+y} : \sqrt{y} = \sqrt{s} : \frac{\sqrt{sy}}{\sqrt{L+y}}$. The fourth term of this analogy expresses the velocity of the piston at the end of the stroke. The quantity of motion produced will be had by multiplying this velocity by the mass L. This gives $\frac{L \times \sqrt{sy}}{\sqrt{L+y}}$; and this divided by the power expended, or by $L+y$, gives us the measure of the performance; namely, $\frac{L\sqrt{sy}}{L+y \times \sqrt{L+y}}$.

That this may be a maximum, consider y as the variable quantity, and make the fluxion of this formula $=o$. This will give us $y=\frac{L}{2}$.

Now $P=L+y, =L+\frac{L}{2}, =\frac{3}{2}L$. Therefore the *whole* load on the outer end of the beam, consisting of the water and the counter-weight, must be two-thirds of the pressure of the atmosphere on the steam-piston.

We have here supposed that the expenditure is the atmospheric pressure; and so it is if we consider it mechanically. But the expenditure of which we are sensible, and which we are anxious to employ to the best advantage, is fuel. Supposing this to be employed with the same judgement in all cases, we are almost entitled, by what we know of the production of steam, to say that the steam produced is proportional to the fuel expended. But the steam requisite for merely filling the cylinder is proportional to the area of the piston, and therefore to the atmospheric pressure. The result of our investigation therefore is still just; but the steam wasted by condensation on the sides of the cylinder does not follow this ratio, and this is more than what is necessary for merely filling it. This deranges our calculations, and is in favour of large cylinders; *but the* *degree in which this circumstance affects the working of the engine has not been accurately determined.*[*]

It must be remarked, that in the preceding investigation we introduced a quantity *m* to express the resistances to the motion of the engine. This was done in order to avoid a very troublesome investigation. The resistances are of such a nature as to vary with the velocity, and most of them as the square of the velocity. This is the case with the resistance arising from the motion of the water through the pistons of the pumps, and that arising from the friction in the long lift during the working-stroke. Had we taken the direct method, which is similar to the determination of the motion through a medium which resists in the duplicate ratio of the velocity, we must have used a very intricate exponential calculus, which few of our readers would have the patience to look at.

But the greatest part of the quantity *m* supposes a motion already known, and its determination depends on this motion. We must now show how its different component parts may be computed.

36. First, What arises from the inertia of the moving parts is by far the most considerable portion of it. To obtain it, we must find a quantity of matter which, when placed at the end of the beam, will have the same momentum of inertia with that of the whole moving parts in their natural places. Therefore (in the returning-stroke) add together the weight of the great piston with its rod and chains; the pit pump-rods, chains, and any weight that is attached to them; the arch-heads and iron-work at the ends of the beam, and $\frac{1}{3}$ths of the weight of the beam itself; also the plug-beam with its arch-head and chain, multiplied by the square of its distance from the axis, and divided by the square of half the length of the beam; also the jack-head pump-rod, chain, and arch-head, multiplied by the square of its distance from the axis, and divided by the square of the half length of the beam. These articles added into one sum may be called M, and may be supposed to move with the velocity of the end of the beam. Suppose this beam to have made a six-foot stroke in two seconds, with an uniformly accelerated motion. In one second it would have moved $1\frac{1}{2}$ feet, and would have acquired the velocity of three feet per second. But in one second gravity would have produced a velocity of 32 feet in the same mass. Therefore the accelerating force, which has produced the velocity of three feet, is nearly $\frac{1}{11}$th of the weight. Therefore $\frac{M}{11}$ is the first constituent of m in the above investigation. If the observed velocity is greater or less than three feet per second, this value must be increased or diminished in the same proportion.

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[*] "In practice, it is a pretty general maxim, as has been said, to load the engines with a column of water equal to about one half of the atmospheric pressure; but by paying attention to the temperature which would produce a degree of vacuum equal to that load and the necessary counter-weight, it will appear that lighter loads will prove more advantageous in point of saving of fuel." W.

The second cause of resistance, viz. the immersion of the pump-rods in water, is easily computed, being the weight of the water which they displace.

The third cause, the friction of the pistons, &c. is almost insignificant, and must be discovered by experiment.

The fourth cause depends on the structure of the pumps. These pumps, when made of a proper strength, can hardly have the perforation of the piston *or bucket* more than a fourth part of the area of the working-barrel; and the velocity with which the water passes through it is increased at least four times by the contraction. The velocity of the water is therefore five times greater than that of the piston. A piston 12 inches diameter, and moving one foot per second, meets with a resistance equal to 20 pounds; and this increases as the square of the diameter and as the square of the velocity. If the whole depth of the pit be divided into several lifts, this resistance must be multiplied by the number of lifts, because it obtains in each pump.

Thus we make up the value of *m*; and we must acknowledge that the method is still indirect, because it supposes the velocity to be known.

We may obtain it more easily in another way, but still with this circumstance of being indirect. We found that p was equal to $\sqrt{Lm}$, and consequently $m = \frac{p^2}{L}$. Now in any engine L and *p* can always be had; and unless *p* deviates greatly from the proportion which we determined to be the best, the value of *m* thus obtained will not be very erroneous.

37. It was farther presumed in this investigation, that the motions both up and down were uniformly accelerated; but this cannot be the case when the resistances increase with the velocity. This circumstance makes very little change in the working-stroke, and therefore the theorem which determines the best relation of P to L may be confided in. The resistances which vary with the velocity in this case are a mere trifle when compared with the moving power *y*. These resistances are, 1st, The strangling of the water at the entry and at the standing valve of each pump: This is about 37 pounds for a pump 12 inches diameter, and the velocity of one foot per second, increasing in the duplicate ratio of the diameter and velocity. And, 2d, The friction of the water along the whole lift: This for a pump of the same size and with the same velocity, lifting 20 fathoms, is only about 2⅓ pounds, and varies in the simple proportion of the diameter and the depth, and in the duplicate proportion of the velocity. The resistance arising from inertia is greater than in the returning-stroke, because the M in this case must contain the momentum of the water both of the pit-pumps and the jackhead-pump: but this part of the resistance does not affect the uniform acceleration. We may therefore confide in the propriety of

the formula $y = \frac{L}{2}$. And we may obtain the velocity of this stroke at the end of a second, with great accuracy, as follows. Let $2g$ be the velocity communicated by gravity in a second, and the velocity at the end of the first second of the steam-piston's descent will be somewhat less than $\frac{y}{M} 2g$; where M expresses the inertia of all the parts which are in motion during the descent of the steam-piston, and therefore includes L. Compute the two resistances just mentioned for this velocity. Call this r. Then $\frac{y - \frac{1}{2} r}{M} 2g$ will give another velocity infinitely near the truth.

But the case is very different in the returning-stroke, and the proper ratio of p to L is not ascertained with the same certainty; for the moving force p is not so great in proportion to the resistance m; and therefore the acceleration of the motion is considerably affected by it, and the motion itself is considerably retarded, and in a very moderate time it becomes sensibly uniform; for it is precisely similar to the motion of a heavy body falling through the air, and may be determined in the manner laid down in the article "Resistance of Fluids," viz. by an exponential calculus. We shall content ourselves here with saying, that the resistances in the present case are so great that the motion would be to all sense uniform before the pistons have descended one-third of their stroke, even although there were no other circumstance to affect it.

38. But this motion is affected by a circumstance quite unconnected with any thing yet considered, depending on conditions not mechanical, and so uncertain, that we are not yet able to ascertain them with any precision; yet they are of the utmost importance to the good performance and improvement of the engine, and therefore deserve a particular consideration.

The counter-weight has not only to push down the pump-rods, but also to *drag* up the great piston. This it cannot do unless the steam be admitted into the cylinder. If the steam be no stronger than common air, it cannot enter the cylinder except *in consequence* of the piston's being dragged up. If common air were admitted into the cylinder, some force would be required to drag up the piston, in the same manner as it is required to draw up the piston of a common syringe; for the air would rush through the small entry of the cylinder in the same manner as through the small nozzle of the syringe. Some part of the atmospheric pressure is employed in driving in the air with sufficient velocity to fill the syringe, and it is only with the remainder that the admitted air presses on the under surface of the syringe. Therefore some of the atmospheric pressure on its upper surface is not balanced. This is felt by the hand which draws it up. The same thing must happen in the steam-engine, and some part of the counter-weight is expended in drawing up the steam-piston. We could tell how much is thus expended if we knew the density of the steam; for this would tell us the velocity with which its elasticity would cause it to fill the cylinder.

But all this is on the supposition that there is an unbounded supply of steam of undiminished elasticity. This is by no means the case. Immediately before opening the steam-cock, the steam was issuing through the safety-valve and all the crevices in the top of the boiler, and (in good engines) was about $\frac{1}{10}$th stronger or more elastic than air. This had been gathering during something more than the descent of the piston, viz. in about three seconds. The piston rises to the top in about two seconds; therefore about twice and a half as much steam as fills the dome of the boiler is now shared between the boiler and cylinder. The dome is commonly about six times more capacious than the cylinder. If therefore no steam is condensed in the cylinder, the density of the steam, when the piston has reached the top, must be about $\frac{15}{16}$ths of its former density, and still more elastic than air. But as much steam is condensed by the cold cylinder, its elasticity must be less than this. We cannot tell how much less, both because we do not know how much is thus condensed, and because, by this diminution of its pressure on the surface of the boiling water, it must be more copiously produced in the boiler; but an attentive observation of the engine will give us some information. The moment the steam-cock is opened we have a strong puff of steam through the snifting-valve. At this time, therefore, it is still more elastic than air; but after this, the snifting-valve remains shut during the whole rise of the piston, and no steam any longer issues through the safety-valve or crevices; nay, the whole dome of the boiler may be observed to sink.

39. These facts give abundant proof that the elasticity of the steam during the ascent of the piston is greatly diminished, and therefore much of the counter-weight is expended in dragging up the steam-piston in opposition to the unbalanced part of the atmospheric pressure. The motion of the returning-stroke is therefore so much deranged by this foreign and inappreciated circumstance, that it would have been quite useless to engage in the intricate exponential investigation, and we must sit down contented with a less perfect adjustment of the counter-weight and weight of water.—Any person who attends to the motion of a steam-engine will perceive that the descent of the pump-rods is so far from being accelerated, that it is nearly uniform, and frequently it is sensibly retarded towards the end. We learn by the way, that it is of the utmost importance not only to have a quick production of steam, but also a very capacious dome, or empty space above the water in the boiler. In engines where this space was but four or five times the capacity of the cylinder, we have always observed a very sensible check given to the descent of the pump-rods after having made half their stroke. This obliges us to employ a greater counter-weight,

which diminishes the column of water, or retards the working-stroke; it also obliges us to employ a stronger steam, at the risk of bursting the boiler, and increases the expence of fuel.

40. It would be a most desirable thing to get an exact knowledge of the elasticity of the steam in the cylinder; and this is by no means difficult. Take a long glass tube exactly calibered, and close at the farther end. Put a small drop of some coloured fluid into it, so as to stand at the middle nearly.—Let it be placed in a long box filled with water to keep it of a constant temperature. Let the open end communicate with the cylinder, with a cock between. The moment the steam-cock is opened, open the cock of this instrument. The drop will be pushed towards the close end of the tube, while the steam in the cylinder is more elastic than the air, and it will be drawn the other way while it is less elastic, and, by a scale properly adapted to it, the elasticity of the steam corresponding to every position of the piston may be discovered. The same thing may be done more accurately by a barometer properly constructed, so as to prevent the oscillations of the mercury.

41. It is equally necessary to know the state of the cylinder during the descent of the steam-piston. We have hitherto supposed P to be the full pressure of the atmosphere on the area of the piston, supposing the vacuum below it to be complete. But the inspection of our table of elasticity shews that this can never be the case, because the cylinder is always of a temperature far above 32°. We have made many attempts to discover its temperature. We have employed a thermometer in close contact with the side of the cylinder, which soon acquired a steady temperature: this was never less than 145°. We have kept a thermometer in the water which lies on the piston: this never sunk below 135°. It is probable that the cylinder within may be cooled somewhat lower; but for this opinion we cannot give any very satisfactory reason. Suppose it cooled down to 120°,

this will leave an elasticity which would support three inches of mercury. We cannot think, therefore, that the unbalanced pressure of the atmosphere exceeds that of 27 inches of mercury, which is about 13½ pounds on a square inch, or 10½ on a circular inch. And this is the value which we should employ in the equation P=L+y. This question may be decided in the same way as the other, by a barometer connected with the inside of the cylinder.*

And thus we shall learn the state of the moving forces in every moment of the performance, and the machine will then be as open to our examination as any water or horse mill; and till this be done, or something equivalent, we can only guess at what the machine is actually performing, and we cannot tell in what particulars we can lend it a helping hand. We are informed that Messrs Watt and Boulton have made this addition to some of their engines; and we are persuaded that, from the information which they have derived from it, they have been enabled to make the curious improvements from which they have acquired so much reputation and profit.

42. There is a circumstance of which we have as yet taken no notice, viz. the quantity of cold water injected. Here we confess ourselves unable to give any precise instructions. It is clear at first sight that no more than is absolutely necessary should be injected. It must generally be supplied by the engine, and this expends part of its power. An excess is much more hurtful by cooling the cylinder and piston too much, and therefore wasting steam during the next rise of the piston. But the determination of the proper quantity requires a knowledge, which we

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* "By examining the hot water which issues from the eduction-pipe of several of Newcomen's engines, its heat was found to vary from 142° to 174°, according to the load and other circumstances of the engine; and the heat of the water discharged at that pipe may be taken as a fair indication of the internal heat of the cylinder." W.

have not yet acquired, of the quantity of heat contained in the steam in a latent form. As much water must be injected as will absorb all this without rising near to the boiling temperature. But it is of much more importance to know how far we may cool the cylinder with advantage; that is, when will the loss of steam, during the next rise of the piston, compensate for the diminution of its elasticity during its present descent? Our table of elasticities shows us, that by cooling the cylinder to 120°, we still leave an elasticity equal to one-tenth of the whole power of the engine; if we cool it only to 140, we leave an elasticity of one-fifth; if we cool it to a blood-heat, we leave an elasticity of one-twentieth. It is extremely difficult to choose among these varieties. Experience, however, informs us, that the best engines are those which use the smallest quantities of injection-water. Mr Watt's observations, by means of the barometer, must have given him much valuable information in this particular, and we hope that he will not withhold them from the public.

43. We have gone thus far in the examination, in order seemingly to ascertain the motion of the engine when loaded and balanced in any known manner, and in order to discover that proportion between the moving power and the load which will produce the greatest quantity of work. The result has been very unsatisfactory, because the computation of the returning-stroke is acknowledged to be beyond our abilities. But it has given us the opportunity of directing the reader's attention to the leading circumstances in this enquiry. By knowing the internal state of the cylinder in machines of very different goodness, we learn the connection between the state of the steam and the performance of the machine; and it is very possible that the result of a full examination may be, that in situations where fuel is expensive, it may be proper to employ a weak steam which will expend less fuel, although less work is perform-

ed by it. We shall see this confirmed in the clearest manner in some particular employments of the new engines invented by Watt and Boulton.

In the mean time, we see that the equation which we gave from the celebrated Abbé Bossut (art. 30), is in every respect erroneous, even for the purpose which he had in view. We also see that the equation which we substituted in its place (art. 32), and which was intended for determining that proportion between the counter-weight and the moving force, and the load which would render the working-stroke and returning-stroke of equal duration, is also erroneous, because these two motions are extremely different in kind, the one being nearly uniform, and the other nearly uniformly accelerated. This being supposed true, it should follow that the counter-weight should be reduced to one half; and we have found this to be very nearly true in some good engines which we have examined.

44. We shall add but one observation more on this head. The practical engineers have almost made it a maxim, that the two motions are of equal duration. But the only reason which we have heard for the maxim is, that it is awkward to see an engine go otherwise. But we doubt exceedingly the truth of this maxim, and, without being able to give any accurate determination, we think that the engine will do more work if the working-stroke be made slower than the returning-stroke. Suppose the engine so constructed that they are made in equal times; an addition to the counter-weight will accelerate the returning-stroke and retard the working-stroke. But as the counter-weight is but small in proportion to the unbalanced portion of the atmospheric pressure, which is the moving force of the machine, it is evident that this addition to the counter-weight must bear a much greater proportion to the counter-weight than it does to the moving force, and must therefore accelerate the returning-stroke much more than it

retards the working-stroke, and the time of both strokes taken together must be diminished by this addition and the performance of the machine improved; and this must be the case as long as the machine is not extravagantly loaded.*

It is needless to engage more deeply in scientific calculations in a subject where so many of the data are so very imperfectly understood.

45. Before quitting this machine, it will not be amiss to give some easy rules, sanctioned by successful practice, for computing its performance. These will enable any artist, who can go through simple calculations, to suit the size of his engine to the task which it is to perform.

The circumstance on which the whole computation must be founded is the quantity of water which must be drawn in a minute, and the depth of the mine; and the performance which may be expected from a good engine, is at least 12 strokes per minute of six feet each, working against a column of water whose weight is equal to half of the atmospheric pressure on the steam-piston, or rather to 7.64 pounds on every square inch of its surface.

It is most convenient to estimate the quantity of water in cubic feet, or its weight in pounds, recollecting that a cubic foot of water weighs 62½ pounds. The depth of the pit is usually reckoned in fathoms of six feet, and the diameter of the cylinder and pump is usually reckoned in inches.

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* "It is now generally agreed, that an engine to work well should make the acting stroke in less time than the returning one, or, in the engine-man's term, should go out slower than it comes in; for, if the buckets of the pumps are defective, as is often the case, the quicker the stroke is taken, the less water will be lost, and the slower the returning-stroke is, the throttling caused by the smallness of the apertures in the bucket of the pump will cause the less resistance to its descent."

Let Q be the quantity of water to be drawn per minute in cubical feet, and f the depth of the mine in fathoms; let c be the diameter of the cylinder, and p that of the pump; and let us suppose the arms of the beam to be of equal length.

1st, To find the diameter of the pump, the area of the piston in square feet is $p^2 \times \frac{0.7854}{144}$. The length of the column drawn in one minute is 12 times 6, or 72 feet, and therefore its solid contents is $p^2 \times \frac{72 \times 0.7854}{144}$ cubical feet, or $p^2 \times 0.3927$ cubical feet. This must be equal to Q; therefore p^2 must be $\frac{Q}{0.3927}$ or nearly $Q \times 2\frac{1}{2}$. Hence this practical rule: Multiply the cubic feet of water which must be drawn in a minute by 2½, and extract the square root of the product: this will be the diameter of the pump in inches.

Thus suppose that 58 cubic feet must be drawn every minute; 58 multiplied by 2½ gives 145, of which the square root is 12, which is the required diameter of the pump.

2. To find the proper diameter of the cylinder.

The piston is to be loaded with 7.64 pounds on every square inch. This is equivalent to six pounds on a circular inch very nearly. The weight of a cylinder of water an inch in diameter and a fathom in height is $2\frac{1}{24}$ pounds, or nearly two pounds. Hence it follows that $6\,c^2$ must be made equal to $2\,f\,p^2$, and that c^2 is equal to $\frac{2\,f\,p^2}{6}$, or to $\frac{f p^2}{3}$.

Hence the following rule: Multiply the square of the diameter of the pump-piston (found as above) by the fathoms of lift, and divide the product by 3; the square root of the quotient is the diameter of the cylinder.

Suppose the pit to which the foregoing pump is to be applied is 24 fathoms deep; then $\frac{24 \times 144}{3}$ gives 1152, of which the square root is 34 inches very nearly.

This engine constructed with care will certainly do the work.

Whatever is the load of *water* proposed for the engine, let 10 be the pounds on every circular inch of the steam-piston, and make $c^2 = p^2 \times \frac{2f}{m}$, and the square root will be the diameter of the steam-piston in inches.

To free the practical engineer as much as possible from all trouble of calculation, we subjoin the following *Table of the Dimensions and Power of the Steam-Engine*, drawn up Mr Beighton in 1717, and fully verified by practice since that time. The measure is in English ale gallons of 282 cubic inches.

Diam. of pump.	Holds in one yard.	Draws by a six-feet stroke.	Weighs in one yard.	At 16 strokes per min.	Ditto in hogs-heads.	Ditto per hour.		The depth to be drawn in yards.											
Inch.	Gall	Gall.	Lb.avoir.	Gall.	Hd. Gal	Hd. Gal.		15	20	25	30	35	40	45	50	60	70	80	90
12	14.4	28.8	146	462	7.21	440	Diameter of cylinder in inches.	18½	21½	24	26½	28½	30½	32½	34¼	37¼	40	43½	
11	12.13	24.26	123.5	438	6.20	369.33		17	19¾	22	25	26	28	29¼	31¼	34½	37	39½	
10	10 02	20.04	102	320	5.5	304.48		15½	18	20	22	23¾	25¼	27	28¾	31¼	34	36	38½
9	8.12	16.24	82.7	259.8	4.7	247.7		14	16¼	18	20	21½	23	24¼	25	28	30½	33	35
8½	7.26	14.52	73.9	232.3	3.43	221.15		13½	15¼	17¼	19	20¼	21¾	23	24	26¼	28½	31	32½
8	6.41	12.82	65.3	205.2	3.16	195.22		12½	14½	16½	18½	19	20½	21	23	25	27	29	30½
7¾	6.01	12.02	61.2	192.3	3.2	182.18		12	14	15½	17½	18¾	19½	21	22	24¼	26	28	29½
7½	5.66	11.32	57.6	181.1	2.55	172.30		11	13¾	15	16½	18	19	20	21¼	23¼	25	27	28½
7	4.91	9.82	50.0	157.1	2.31	149.40		10¾	13	14	15½	16¾	18¾	19	20½	22	24	25	27
6½	4.23	8.46	43	135.3	2.9	128.54		10	12	13	14	15½	16¼	18	19	20	22	23	24½
6	3.61	7.2	36.7	115.5	1.52	110.1		9½	11	12	13	14	15½	16	17	19	20½	22	23
5½	3.13	6.2	31.8	99.2	1.36	94.30			10	11	12	13	14	15	15¾	17	19	20	21
5	2.51	5 0	25.5	80 3	1.7	66.61				10	11	11¾	13	13¼	14	15½	16¾	18½	19½
4½	2.02	4.04	20.5	64 6	1.1	60.60					10	11	11¾	12	13½	14	15	16	17
4	1.6	3.2	16.2	51.2	0.51	48.51						9	10	11	11½	12	13½	14	15

46. The first part of the table gives the size of the pump suited to the growth of water. The second gives the size of the cylinder suited to the load of water. If the depth is greater than any in this table, take its fourth part, and double the diameter of the cylinder. Thus, if 150 hogsheads are to be drawn in an hour from the depth of 100 fathoms, the last column of part first gives for 149.40 a pump of seven inches bore. In a line with this, under the depth of 50 yards, which is one-fourth of 100 fathoms, we find 20½, the double of which is 41 inches for the diameter of the cylinder.

It is almost impossible to give a general rule for strokes of different lengths, &c., but one who professes the ability to erect an engine, should surely know as much arithmetic as will accommodate the rule now given to any length of stroke.

We venture to say, that no ordinary engineer can tell *à priori* the number per minute which an engine will give. We took twelve strokes of six feet each for a standard, which a careful engineer may easily accomplish, and which an employer has a right to expect, the engine being loaded with water to half the pressure of the atmosphere: if the the load be less, there is some fault; an improper counterweight, or too little boiler, or leaks, &c. &c.

47. Such is the state in which Newcomen's steam-engine had continued in use for 60 years, neglected by the philosopher, although it is the most curious object which human ingenuity has yet offered to his contemplation, and abandoned to the efforts of the unlettered artist. Its use has been entirely confined to the raising of water. Mr Keane Fitzgerald, indeed, published, in the Philosophical Transactions in 1758, a method of converting its reciprocating motion into a continued rotatory motion by employing *a combination of large-toothed wheels, and of smaller ratchet-wheels, worked by teeth upon the arch or sector of the great beam. One of these ratchet-wheels being put in motion by the*

ascent of the beam, and standing still during its descent, when another ratchet-wheel is moved by an intervening wheel in the same direction as the first, and thus the two communicate a continued rotative motion to the axis on which they are placed, which is thence transmitted by a large-toothed wheel to a smaller wheel or pinion, on the shaft of which is a fly, to accumulate momentum, and a crank proposed to be applied to work ventilators, and to many other useful purposes. The fly, by accumulating in itself the power of the machine during the time it was acted upon, would continue in motion, and urge forward the machinery whilst the steam-engine was going through its inactive returning-stroke. This will be the case, provided that the resistance exerted by the working-machine during the whole period of the working and returning-stroke of the steam-engine, together with the friction of both, does not exceed the whole pressure exerted by the steam-engine during its working-stroke; and provided that the momentum of the fly, arising from its great weight and velocity, be very great, so that the resistance of the work during one returning-stroke of the steam-engine do not make any very sensible diminution of the velocity of the fly. This is evidently possible and easy. The fly may be made of any magnitude; and being exactly balanced round its axis, it will soon acquire any velocity consistent with the motion of the steam-engine. During the working-stroke of the engine it is uniformly accelerated, and by its acquired momentum it produces in the beam the movement of the returning-stroke; but in doing this, its momentum is shared with the inert matter of the steam-engine, and consequently its velocity diminished, but not entirely taken away. The next working-stroke, therefore, by pressing on it afresh, increases its remaining velocity by a quantity nearly equal to the whole that it acquired during the first stroke. We say *nearly*, but not quite equal, because the time of the second working-stroke must be shorter than that of the first, on account of the velocity already in the machine. In this manner the fly will be more and more accelerated every succeeding stroke, because the pressure of the engine during the working-stroke does more than restore to the fly the momentum which it lost in producing the returning movement of the steam-engine. Now suppose the working part of the machine to be added. The acceleration of the fly during each working-stroke of the steam-engine will be less than it was before, because the impelling pressure is now partly employed in driving the working-machine, and because the fly will lose more of its momentum during the returning-stroke of the steam-engine, part of it being expended in driving the working-machine. It is evident, therefore, that a time will come when the successive augmentation of the fly's velocity will cease; for, on the one hand, the continual acceleration diminishes the time of the next working-stroke, and therefore the time of action of the accelerating power. The acceleration must diminish in the same proportion; and, on the other hand, the resistance of the working-machine generally, though not always, increases with its velocity. The acceleration ceases whenever the addition made to the momentum of the fly during a working-stroke of the steam-engine is just equal to what it loses by driving the machine, and by producing the returning movement of the steam-engine.

48. This must be acknowledged to be a very important addition to the engine, and though sufficiently obvious, it is ingenious, and requires considerable skill and address to make it effective.*

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* We do not recollect at present the date of this proposal of Mr Fitzgerald; but in 1781 the Abbé Arnal, canon of Alais, in Languedoc, entertained a thought of the same kind, and proposed it for working lighters in the inland navigations; a scheme which has been successfully practised (we are told) in America. His brother, a major of engineers in the Austrian service, has carried the thing much farther, and applied it to manufactures; and the Aulic Chamber of Mines at Vienna has patronised the project. (See *Journal Encyclopedique*, 1781.) But these schemes are long posterior to Mr Fitzgerald's patent, and are even later than the erection of several machines driven

The movement of the working-machine, or mill of whatever kind, must be in some degree hobbling or unequal. But this may be made quite insensible, by making the fly exceedingly large, and disposing the greatest part of its weight in the rim. By these means its momentum may be made so great, that the whole force required for driving the mill and producing the returning movement of the engine, may bear a very small proportion to it. The diminution of its velocity will then be very trifling.

No counter-weight is absolutely necessary here, because the returning movement is produced by the inertia of the fly. A counter-weight may, however, be employed, and should be employed, viz. as much as will produce the returning movement of the steam-engine. It will do this better than the same force accumulated in the fly; for this force must be accumulated in the fly by the intervention of rubbing parts, by which some of it is lost; and it must be afterwards returned to the engine with a similar loss. But, for the same reason, it would be improper to make the counter-weight also able to drive the mill during the returning stroke.

49. By this contrivance Mr Fitzgerald hoped to render the steam-engine of most extensive use; and he, or others associated with him, obtained a patent excluding all others from employing *his invention*. They also published proposals for erecting mills of all kinds driven by steam-engines, and stated very fairly their powers and their advantages. But their proposals do not seem to have acquired the confidence of the public; for we do not know of any mill ever having been erected under this patent.*

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by steam-engines which have been erected by Messrs Watt and Boulton. We think it our duty to state these particulars, because it is very usual for our neighbours on the continent to assume the credit of British inventions. Dr R.

* "It would seem, from Mr Fitzgerald's own account in the Philosophical Transactions, that he had made a model of it, but he does not say it had been in actual use; nor is it likely, from its extreme complexity and other defects,

50. The great obstacle to this extensive use of the steam-engine, is the prodigious expense of fuel. An engine having a cylinder of four feet diameter, working night and day, consumes about 3400 chaldron (London) of good coals in a year.

51. This circumstance limits the use of steam-engines exceedingly. To draw water from coal-pits where they can be stocked with unsaleable small coal, they are of universal employment: also for valuable mines, for supplying a great and wealthy city with water, and a few other purposes, where a great expense can be borne, they are very proper engines; but in a thousand cases, where their unlimited powers might be vastly serviceable, the enormous expense of fuel completely excludes them. We cannot doubt but that the attention of engineers was much directed to every thing that could promise a diminution of this expense. Every one had his particular nostrum for the construction of his furnace, and some were undoubtedly more successful than others. But science was not yet sufficiently advanced: It was not till Dr Black had made his beautiful discovery of latent heat, that we could know the intimate relation between the heat expended in boiling off a quantity of water and the quantity of steam that is produced.

Much about the time of this discovery, viz. 1763-4, Mr

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that, had it ever been carried into practice, the use of it would have been persevered in. Upon an accurate search at the different offices, it does not appear that any patent was taken out by Fitzgerald, nor have I any knowledge of the proposals mentioned in the text.

"On the application of the crank to produce rotative motions from the steam-engine, I shall treat more at length in describing the new engine.

"Some imperfect and unsuccessful attempts were made in the years 1767 and 1769, by Mr John Stewart, and Mr Dugald Clarke, to derive a continued rotative motion from Newcomen's engine, to be applied to sugar-mills in Jamaica; and something of the kind was attempted at Hartley colliery, near Newcastle. The apparatus of all of them appears to have been complex, liable to breakages and derangements, and not susceptible of regularity; in consequence of which these schemes were, I believe, soon abandoned." We

James Watt, established in Glasgow *as a mathematical instrument maker, was employed to repair* a working model of the steam-engine which belonged to the philosophical apparatus of the university. Mr Watt was a person of a truly philosophical mind, eminently conversant in all branches of natural knowledge, and the friend of Dr Black.

52. In the course of *these repairs*, many curious facts in the production and condensation of steam occurred to him; and, among others, that remarkable fact which is always appealed to by Dr Black as the proof of the immense quantity of heat which is contained in a very minute quantity of water in the form of elastic steam. When a quantity of water is heated several degrees above the boiling point in a close digester, if a hole be opened, the steam rushes out with prodigious violence, and the heat of the remaining water is reduced, in the course of three or four seconds, to the boiling temperature. The water of the steam which has issued amounts only to a very few *ounces*; and yet these have carried off with them the whole excess of heat from the water in the digester.*

53. Since then a certain quantity of steam contains so great a quantity of heat, it must expend a great quantity of

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* "When the digester was set upon a steady fire for a given time, half an hour for instance, the steam being allowed to issue freely by keeping the safety-valve quite *open*, and the quantity of water evaporated in that time being ascertained; if the digester was again placed on the fire, and continued for an equal time with the safety-valve *shut*, upon opening that valve a quantity of steam would issue with violence; and when the elasticity of the steam issuing was reduced to that of the atmosphere, I found the quantity of water evaporated in these circumstances was apparently the same as had evaporated in an equal time when the valve was constantly open: From whence I concluded, that the quantities of water evaporated in any given time, were proportional to the quantity of heat which entered it *et cæteris paribus* to the surfaces exposed to the fire, and not to the surfaces exposed to the air, as had been supposed, and as is the case where the air is the sole agent of evaporation in heats below boiling. (See Dr Black's Lectures, articles Latent Heat, and Improvements on the Steam-Engine, p. 100, et seq.)" W.

fuel; and no construction of furnace can prevent this. Mr Watt, therefore, set his invention to work to discover methods of husbanding this heat. The cylinder of his little model was heated almost in an instant, so that it could not be touched by the hand. It could not be otherwise, because it condensed the vapour by abstracting its heat. But all the heat thus communicated to the cylinder, and wasted by it on surrounding bodies, contributed nothing to the performance of the engine, and must be taken away at every injection, and again communicated and wasted. Mr Watt quickly understood the whole process which was going on within the cylinder, and which we have considered so minutely, and saw that a very considerable portion of the steam must be wasted in warming the cylinder. His first attempts were made to ascertain how much was thus wasted, and he found that it was *many* times as much as would fill the cylinder and work the engine. He attempted to diminish this waste by using wooden cylinders. But though this produced a sensible diminution of the waste, other reasons forced him to give them up. He then *surrounded thin* metal cylinders *with* a wooden case. By this, and using no more injection than was absolutely necessary for the condensation, he reduced the waste *considerably*; and there consequently remained in it a steam of very considerable elasticity, which robbed the engine of a proportional part of the atmospherical pressure. He saw that this was unavoidable as long as the condensation was performed in the cylinder.

54. The thought struck him to attempt the condensation in another place. His first experiment was made in the simplest manner.* A globular vessel communicated by

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* "The globular vessel only existed in Mr W.'s mind, and was never executed. The tin cylinder is a mistake; there never was any used in this model, the cylinder being of brass. The crushing relates to another experiment on the thickness of cylinders necessary to resist the pressure of the atmosphere." W.

means of a long pipe of one inch diameter, with the bottom of his little cylinder of four inches diameter and 30 inches long. This pipe had a stop-cock, and the globe was immersed in a vessel of cold water. When the piston was at the top, and the cylinder filled with strong steam, he turned the cock. It was scarcely turned, nay he did not think it completely turned, when the sides of his cylinder (only strong tin-plate) were crushed together like an empty bladder. This surprised and delighted him. A new cylinder was immediately made of brass, sufficiently thick, and nicely bored. When the experiment was repeated with this cylinder, the condensation was so rapid, that he could not say that any time was expended in it. But the most valuable discovery was, that the vacuum in the cylinder was, as he hoped, almost perfect. Mr Watt found, that when he used water in the boiler purged of air by long boiling, nothing that was very sensibly inferior to the pressure of the atmosphere on the piston could hinder it from coming quite down to the bottom of the cylinder. This alone was gaining a great deal, for in most engines the remaining elasticity of the steam was not less than one-eighth of the atmospherical pressure, and therefore took away one-eighth of the power of the engine.

55. Having gained this capital point, Mr Watt found many difficulties to struggle with before he could get the machine to continue its motion. The water produced from the condensed steam, and the air which was extricated from it, or which penetrated through unavoidable leaks, behoved to accumulate in the condensing vessel, and could not be avoided in any way similar to that adopted in Newcomen's engine. He took another method: He applied pumps to extract both, which were worked by the great beam. The contrivance is easy to any good mechanic; only we must observe, that the piston of the water-pump must be under the surface of the water in the condenser, that the water

may enter the pump by its own weight, because there is no atmospherical pressure there to force it in. We must also observe, that a considerable force is necessarily expended here, because, as there is but one stroke for rarefying the air, and this rarefaction must be nearly complete, the air-pump must be of large dimensions, and its piston must act against the whole pressure of the atmosphere. Mr Watt, however, found that this force could be easily spared from his machine, already so much improved in respect of power.

56. Thus has the steam-engine received a very considerable improvement. The cylinder may be allowed to remain very hot; nay, boiling hot, and yet the condensation be completely performed. The only elastic steam that now remains is the small quantity in the pipe of communication. Even this small quantity Mr Watt at last got rid of, by admitting a small jet of cold water up this pipe to meet the steam in its passage to the condenser. This both cooled this part of the apparatus in a situation where it was not necessary to warm it again, and it quickened the condensation. He found at last that the small pipe of communication was of itself sufficiently large for the condensation, and that no separate vessel, under the name of condenser, was necessary. This circumstance shows the prodigious rapidity of the condensation. We may add, that unless this had been the case, his improvement would have been vastly diminished; for a large condenser would have required a much larger air-pump,* which would have expended much of the power of the engine. By these means the vacuum below the piston is greatly improved: For it will appear

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* "This is not correct; for though the same sized pump would be longer in exhausting a larger condenser, (were it employed for that purpose) yet the same degree of exhaustion would be maintained were it regularly at work, however large the condenser might be; and a larger condenser causes the commencement of the exhaustion to be more speedy than a smaller one does. The size of the air-pump has relation properly to the quantity of air leakage, and not to the capacity of the condenser." W.

clear to any person who understands the subject, that as long as any part of the condenser is kept of a low temperature, it will abstract and condense the vapour from the warmer parts, till the whole acquires the elasticity corresponding to the coldest part. By the same means much of the waste is prevented, because the cylinder is never cooled much below the boiling temperature. Many engines have been erected by Mr Watt in this form, and their performance gave universal satisfaction.

We have contented ourselves with giving a very slight description without a figure of this improved engine, because we imagine it to be of very easy comprehension, and because it is only a preparation for still greater improvements, which, when understood, will at the same time leave no part of this more simple form unexplained.

57. During the progress of these improvements, Mr Watt made many experiments on the quantity and density of the steam of boiling water. These fully convinced him, that although he had greatly diminished the waste of steam, a great deal yet remained, and that the steam expended during the rise of the piston was at least three times more than what would fill the cylinder. The cause of this was very apparent. In the subsequent descent of the piston, covered with water much below the boiling temperature, the whole cylinder was necessarily cooled and exposed to the air.* Mr Watt's fertile genius immediately suggested to him the expedient of employing the elasticity of the steam from the boiler to impel the piston down the cylinder, in place of the pressure of the atmosphere; and thus he restored the engine to its first principles, making it an en-

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* "This is a mistake. From the first, I proposed to act upon the piston with steam instead of the atmosphere, and my model was so constructed." W.

gine *really moved by steam.*[*] As this is a new epoch in its history, we shall be more particular in the description;

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[*] "The account of this invention in the text not being perfectly correct, I subjoin the following short history of it:—W.

"My attention was first directed in the year 1759 to the subject of steam-engines, by the late Dr Robison himself, then a student in the University of Glasgow, and nearly of my own age. He at that time threw out an idea of applying the power of the steam-engine to the moving of wheel-carriages, and to other purposes, but the scheme was not matured, and was soon abandoned on his going abroad.

"About the year 1761, or 1762, I tried some experiments on the force of steam, in a Papin's digester, and formed a species of steam-engine by fixing upon it a syringe one-third of an inch diameter, with a solid piston, and furnished also with a cock to admit the steam from the digester, or shut it off at pleasure, as well as to open a communication from the inside of the syringe to the open air, by which the steam contained in the syringe might escape. When the communication between the digester and syringe was opened, the steam entered the syringe, and by its action upon the piston raised a considerable weight (15 lb.) with which it was loaded. When this was raised as high as was thought proper, the communication with the digester was shut, and that with the atmosphere opened; the steam then made its escape, and the weight descended. The operations were repeated, and though in this experiment the cock was turned by hand, it was easy to see how it could be done by the machine itself, and to make it work with perfect regularity. But I soon relinquished the idea of constructing an engine upon this principle, from being sensible it would be liable to some of the objections against Savary's engine, viz. the danger of bursting the boiler, and the difficulty of making the joints tight, and also that a great part of the power of the steam would be lost, because no vacuum was formed to assist the descent of the piston.[†]

"The attention necessary to the avocations of business prevented me from then prosecuting the subject farther; but in the winter of 1763-4, having occasion to repair a model of Newcomen's engine belonging to the natural philosophy class of the University of Glasgow, my mind was again directed to it. At that period, my knowledge was derived principally from Desaguliers, and partly from Belidor. I set about repairing it as a mere mechanician,

[†] "I, however, described this engine in the fourth article of the specification of my patent of 1769; and again in the specification of another patent in the year 1784, together with a mode of applying it to the moving of wheel-carriages." W.

at the same time still restricting ourselves to the essential circumstances, and avoiding every peculiarity which is to

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and when that was done and it was set to work, I was surprised to find that its boiler could not supply it with steam, though apparently quite large enough; (the cylinder of the model being two inches in diameter, and six inches stroke, and the boiler about nine inches diameter.) By blowing the fire it was made to take a few strokes, but required an enormous quantity of injection water, though it was very lightly loaded by the column of water in the pump. It soon occurred that this was caused by the little cylinder exposing a greater surface to condense the steam than the cylinders of larger engines did in proportion to their respective contents. It was found that by shortening the column of water in the pump, the boiler could supply the cylinder with steam, and that the engine would work regularly with a moderate quantity of injection. It now appeared that the cylinder of the model being of brass, would conduct heat much better than the cast-iron cylinders of larger engines, (generally covered on the inside with a stony crust) and that considerable advantage could be gained by making the cylinders of some substance that would receive and give out heat slowly: Of these, wood seemed to be the most likely, provided it should prove sufficiently durable. A small engine was therefore constructed with a cylinder six inches diameter, and twelve inches stroke, made of wood, soaked in linseed oil, and baked to dryness. With this engine many experiments were made; but it was soon found that the wooden cylinder was not likely to prove durable, and that the steam condensed in filling it still exceeded the proportion of that required for large engines according to the statements of Desaguliers. It was also found, that all attempts to produce a better exhaustion by throwing in more injection, caused a disproportionate waste of steam. On reflection, the cause of this seemed to be the boiling of water in vacuo at low heats, a discovery lately made by Dr Cullen, and some other philosophers, (below 100°, as I was then informed) and, consequently, at greater heats, the water in the cylinder would produce a steam which would, in part, resist the pressure of the atmosphere.

" By experiments which I then tried upon the heats at which water boils under several pressures greater than that of the atmosphere, it appeared, that when the heats proceeded in an arithmetical, the elasticities proceeded in some geometrical ratio; and by laying down a curve from my data, I ascertained the particular one near enough for my purpose. It also appeared, that any approach to a vacuum could only be obtained by throwing in large quantities of injection, which would cool the cylinder so much as to require quantities of steam to heat it again, out of proportion to the power gained by the more perfect vacuum; and that the old engineers had acted wisely in contenting themselves with loading the engine with only six or seven pounds on each square inch of the area of the piston. It being evident be found in the prodigious varieties which Mr Watt has introduced into the machines which he has erected, every

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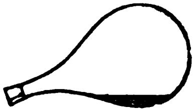
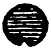
that there was a great error in Dr Desaguliers' calculations of Mr Beighton's experiments on the bulk of steam, a Florence flask, capable of containing about a pound of water, had about one ounce of distilled water put into it; a glass tube was fitted into its mouth, and the joining made tight by lapping that part of the tube with packthread covered with glaziers' putty. When the flask was set upright, the tube reached down near to the surface of the water, and in that position the whole was placed in a tin reflecting oven before a fire, until the water was wholly evaporated, which happened in about an hour, and might have been done sooner had I not wished the heat not much to exceed that of boiling water. As the air in the flask was heavier than the steam, the latter ascended to the top, and expelled the air through the tube. When the water was all evaporated, the oven and flask were removed from the fire, and a blast of cold air was directed against one side of the flask, to collect the condensed steam in one place. When all was cold, the tube was removed, the flask and its contents were weighed with care; and the flask being made hot, it was dried by blowing into it by bellows, and when weighed again, was found to have lost rather more than four grains, estimated at 4½ grains. When the flask was filled with water, it was found to contain about 17½ ounces avoirdupois of that fluid, which gave about 1800 for the expansion of water converted into steam of the heat of boiling water.

"This experiment was repeated with nearly the same result; and in order to ascertain whether the flask had been wholly filled with steam, a similar quantity of water was for the third time evaporated; and, while the flask was still cold, it was placed inverted, with its mouth (contracted by the tube) immersed in a vessel of water, which it sucked in as it cooled, until in the temperature of the atmosphere it was filled to within half an ounce measure of water.†

"In repetitions of this experiment at a later date, I simplified the apparatus by omitting the tube, and laying the flask upon its side in the oven, thus, partly closing its mouth by a cork, having a notch on one side, thus, and otherwise proceeding as has been mentioned.





"I do not consider these experiments as extremely accurate, the only scale-individual of which has been adapted to local circumstances, or diversified by the progress of Mr Watt's improvements.

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† "In the contrivance of this experiment I was assisted by Dr Black.

"In Dr Robison's edition of Dr Black's Lectures, vol. I. page 147, the latter hints at some experiments upon this subject as made by *him*; but I have no knowledge of any except those which I made myself."

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beam of a proper size which I had then at my command, not being very sensible, and the bulk of the steam being liable to be influenced by the heat to which it is exposed, which, in the way described, is not easily regulated or ascertained; but, from my experience in actual practice, I esteem the expansion to be rather more than I have computed.

"A boiler was constructed which showed, by inspection, the quantity of water evaporated in any given time, and thereby ascertained the quantity of steam used in every stroke by the engine, which I found to be several times the full of the cylinder. Astonished at the quantity of water required for the injection, and the great heat it had acquired from the small quantity of water in the form of steam which had been used in filling the cylinder, and thinking I had made some mistake, the following experiment was tried:—A glass tube was bent at right angles, one end was inserted horizontally into the spout of a tea-kettle, and the other part was immersed perpendicularly in well-water contained in a cylindric glass vessel, and steam was made to pass through it until it ceased to be condensed, and the water in the glass vessel was become nearly boiling hot. The water in the glass vessel was then found to have gained an addition of about one-sixth part from the condensed steam. Consequently, water converted into steam can heat about six times its own weight of well-water to 212°, or till it can condense no more steam. Being struck with this remarkable fact, and not understanding the reason of it, I mentioned it to my friend Dr Black, who then explained to me his doctrine of latent heat, which he had taught for some time before this period, (summer 1764,) but having myself been occupied with the pursuits of business, if I had heard of it, I had not attended to it, when I thus stumbled upon one of the material facts by which that beautiful theory is supported.

"On reflecting further, I perceived, that in order to make the best use of steam, it was necessary, First, that the cylinder should be maintained always as hot as the steam which entered it; and, Secondly, that when the steam was condensed, the water of which it was composed, and the injection itself, should be cooled down to 100°, or lower, where that was possible. The means of accomplishing these points did not immediately present themselves; but early in 1765 it occurred to me, that if a communication were opened between a cylinder containing steam, and another vessel which was exhausted of air and other fluids, the steam, as an elastic fluid, would immediately rush into the empty vessel, and continue so to do until it had established an equilibrium; and if that vessel were kept very cool by an injection, or otherwise, more steam would continue to enter until the whole was condensed. But both the vessels being exhausted, or nearly so, how was the injection-

58. Let A (Plate I. fig. 9.) represent the boiler. This has received great improvements from his complete acquaintance

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water, the air which would enter with it, and the condensed steam, to be got out? This I proposed, in my own mind, to perform in two ways. One was by adapting to the second vessel a pipe reaching downwards more than 34 feet, by which the water would descend, (a column of that length overbalancing the atmosphere) and by extracting the air by means of a pump.

" The second method was by employing a pump, or pumps, to extract both the air and the water, which would be applicable in all places, and essential in those cases where there was no well or pit.

" This latter method was the one I then preferred, and is the only one I afterwards continued to use.

" In Newcomen's engine, the piston is kept tight by water, which could not be applicable in this new method; as, if any of it entered into a partially-exhausted and hot cylinder, it would boil and prevent the production of a vacuum, and would also cool the cylinder by its evaporation during the descent of the piston. I proposed to remedy this defect by employing wax, tallow, or other grease, to lubricate and keep the piston tight. It next occurred to me, that the mouth of the cylinder being open, the air which entered to act on the piston would cool the cylinder, and condense some steam on again filling it, I therefore proposed to put an air-tight cover upon the cylinder, with a hole and stuffing-box for the piston-rod to slide through,† and to admit steam above the piston to act upon it instead of the atmosphere. There still remained another source of the destruction of steam, the cooling of the cylinder by the external air, which would produce an internal condensation whenever steam entered it, and which would be repeated every stroke; this I proposed to remedy by an external cylinder containing steam, surrounded by another of wood, or of some other substance which would conduct heat slowly.

" When once the idea of the separate condensation was started, all these improvements followed as corollaries in quick succession, so that in the course of one or two days, the invention was thus far complete in my mind, and I immediately set about an experiment to verify it practically. I took a large brass syringe, 1¾ inches diameter, and 10 inches long, made a cover and botwith the procedure of nature in the production of steam.

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† N. B. " The piston-rod sliding through a stuffing-box was new in steam-engines; it was not necessary in Newcomen's engine, as the mouth of the cylinder was open, and the piston stem was square and very clumsy. The fitting the piston-rod to the piston by a cone was an after improvement of mine (about 1774.")

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tom to it of tin-plate, with a pipe to convey steam to both ends of the cylinder from the boiler; another pipe to convey steam from the upper end to the condenser (for, to save apparatus, I inverted the cylinder.) I drilled a hole longitudinally through the axis of the stem of the piston, and fixed a valve at its lower end, to permit the water which was produced by the condensed steam on first filling the cylinder, to issue. The condenser used upon this occasion consisted of two pipes of thin tin-plate, ten or twelve inches long, and about one-sixth inch diameter, standing perpendicular, and communicating at top with a short horizontal pipe of large diameter, having an aperture on its upper side which was shut by a valve opening upwards. These pipes were joined at bottom to another perpendicular pipe of about an inch diameter, which served for the air and water-pump; and both the condensing pipes and the air-pump were placed in a small cistern filled with cold water.‡

" The steam-pipe was adjusted to a small boiler. When steam was produced, it was admitted into the cylinder, and soon issued through the perforation of the rod, and at the valve of the condenser. When it was judged that the air was expelled, the steam-cock was shut, and the air-pump piston-rod was drawn up, which leaving the small pipes of the condenser in a state of vacuum, the steam entered them and was condensed. The piston of the cylinder immediately rose and lifted a weight of about 18 lbs., which was hung to the lower end of the piston-rod. The exhaustion-cock was shut, the steam was readmitted into the cylinder, and the operation was repeated, the quantity of steam consumed, and the weights it could raise were observed, and, excepting the non-application of the steam-case and external covering, the invention was complete, in so far as regarded the savings of steam and fuel. A large model, with an outer cylinder and wooden case, was immediately constructed, and the experiments made with it served to verify the expectations I had formed, and to place the advantage of the invention beyond the reach of doubt. It was found convenient afterwards to change the pipe-condenser for an empty vessel, generally of a cylindrical form, into which an injection played, and in consequence of there being more water and air to extract, to enlarge the air-pump.

" The change was made, because, in order to procure a surface sufficiently extensive to condense the steam of a large engine, the pipe-condenser would

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‡ " N. B. This construction of the condenser was employed from knowing that heat penetrated thin plates of metal very quickly, and considering that if no injection was thrown into an exhausted vessel, there would be only the water of which the steam had been composed, and the air which entered with the steam, or through the leaks, to extract." W.

In some of his engines the fuel has been placed in the midst

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require to be very voluminous, and because the bad water with which engines are frequently supplied, would crust over the thin plates, and prevent their conveying the heat sufficiently quick. The cylinders were also placed with their mouths upwards, and furnished with a working-beam, and other apparatus, as was usual in the ancient engines; the inversion of the cylinder, or rather of the piston-rod, in the model, being only an expedient to try more easily the new invention, and being subject to many objections in large engines.

"In 1768 I applied for letters patent for my 'Methods of Lessening the Consumption of Steam, and consequently of Fuel, in Fire-Engines,' which passed the seals in January 1769, and my Specification was enrolled in Chancery in April following, and was as follows:—

"My method of lessening the consumption of steam, and consequently fuel, in fire-engines, consists of the following principles:

"First, That vessel in which the powers of steam are to be employed to work the engine, which is called the cylinder in common fire-engines, and which I call the steam-vessel, must, during the whole time the engine is at work, be kept as hot as the steam that enters it; first, by enclosing it in a case of wood, or any other materials that transmit heat slowly; secondly, by surrounding it with steam, or other heated bodies; and, thirdly, by suffering neither water, or any other substance colder than the steam, to enter or touch it during that time.

"Secondly, In engines that are to be worked wholly or partially by condensation of steam, the steam is to be condensed in vessels distinct from the steam vessels or cylinders, although occasionally communicating with them; these vessels I call condensers; and, whilst the engines are working, these condensers ought at least to be kept as cold as the air in the neighbourhood of the engines, by application of water, or other cold bodies.

"Thirdly, Whatever air or other elastic vapour is not condensed by the cold of the condenser, and may impede the working of the engine, is to be drawn out of the steam-vessels or condensers by means of pumps, wrought by the engines themselves, or otherwise.

"Fourthly, I intend in many cases to employ the expansive force of steam to press on the pistons, or whatever may be used instead of them, in the same manner as the pressure of the atmosphere is now employed in common fire-engines: In cases where cold water cannot be had in plenty, the engines may be wrought by this force of steam only, by discharging the steam into the open air after it has done its office.

"Fifthly, where motions round an axis are required, I make the steam-vessels in form of hollow rings, or circular channels, with proper inlets and outlets for the steam, mounted on horizontal axles, like the wheels of a water-mill; within them are placed a number of valves, that suffer any body of the water, surrounded by an iron or copper vessel, while the exterior boiler was made of wood, which transmits, and therefore wastes the heat very slowly.* In others, the flame

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to go round the channel in one direction only; in these steam-vessels are placed weights, so fitted to them as entirely to fill up a part or portion of their channels, yet rendered capable of moving freely in them, by the means hereinaftermentioned or specified: When the steam is admitted in these engines, between these weights and the valves, it acts equally on both, so as to raise the weight to one side of the wheel, and by the reaction on the valves, successively to give a circular motion to the wheel, the valves opening in the direction in which the weights are pressed, but not in the contrary; as the steam-vessel moves round, it is supplied with steam from the boiler, and that which has performed its office may either be discharged by means of condensers, or into the open air.

"Sixthly, I intend, in some cases, to apply a degree of cold, not capable of reducing the steam to water, but of contracting it considerably, so that the engines shall be worked by the alternate expansion and contraction of the steam.

"Lastly, Instead of using water to render the piston or other parts of the engines air and steam-tight, I employ oils, wax, resinous bodies, fat of animals, quicksilver, and other metals, in their fluid state.

"And the said James Watt, by a memorandum added to the said specification, declared, that he did not intend that any thing in the fourth article should be understood to extend to any engine where the water to be raised enters the steam-vessel itself, or any vessel having an open communication with it." W.

* "The exterior part of large boilers was never executed in wood by me; this relates only to some of my models, and one or two very small engines which I made of Newcomen's kind. Wood is improper, because it softens by the steam, and finally gives way. The conveying the flame through flues in the inside of the water had been practised by others before my time, and was common in the Cornish engines. The inventor is unknown, but a person of the name of Swaine was a great propagator of the practice; however, I somewhat improved the form and adjusted the proportions. The property of consuming the smoke was not derived from the construction of the flues, nor from the extensive surface to which the fire was applied, but from another contrivance of mine, called the smokeless furnace, which proceeds somewhat upon the principle of Argand's lamps. The grate and dead-plates are laid sloping downwards from the fire-door, at an angle of about 25 to 30° to the horizon. The fire is lighted as usual, and a small quantity of air is admitted through one or two openings in the fire door, so as to blow directly on the blazing part of the fire. The fire is made at first principally near the dead-plate, and the fresh coals with which it is to be supplied, are laid upon that

not only plays round the whole outside, as in common boilers, but also runs along several flues which are conducted through the midst of the water. By such contrivances the fire is applied to the water in a most extensive surface, and for a long time, so as to impart to it the greatest part of its heat. So skilfully was it applied in the Albion Mills, that although it was a large engine, its unconsumed smoke was inferior to that of a very small brewhouse.* In this second engine of Mr Watt, the top of the cylinder is shut up by a strong metal plate *g h*, fig. 9. in the middle of which is a collar or box *k l*, containing a collar of hemp, surrounding the piston-rod PD, which being nicely turned and polished, can move up and down, without allowing any air to pass by its sides. From the dome of the boiler proceeds a large pipe BCIOQ, which, after reaching the cylinder with its horizontal part BC, descends parallel to its side, sending off two branches, viz. IM to the top of the cylinder, and ON to its bottom. At I is a puppet-valve opening from below upwards. At L, immediately

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plate, close to the burning fuel, but not upon it. When it needs mending, the burning coals and those upon the dead-plate are pushed further down without being mixed, and more coals are laid upon the dead-plate, but never should be thrown on the top of those already on fire, as that would instantly send out a volume of smoke. In this situation they are *gradually* dried, and any smoke which issues from them is consumed by the current of air from the fire door in passing over the bright burning fuel. The opening, or openings, to admit the air, are regulated, so as just to admit the quantity which consumes the smoke; more would be prejudicial. I originally constructed these furnaces in a somewhat different manner; but the above method has been found the most convenient, and, when properly attended to, answers the purpose perfectly with free-burning coal, but is more difficult to manage with coal which cakes." W.

* "The engine here described is one applicable to the working on the expansive principle, in which the piston ascends in an exhausted cylinder; a vacuum both above and below the piston. To make it ascend in steam, the place of the injection must be altered, so as not to spout so high; and there must be a regulating valve at U, to prevent the steam going into the eduction-pipe until the piston has ascended to the top of the cylinder." W.

below this branch, there is a similar valve, also opening from below upwards. The pipe descends to Q, near the bottom of a large cistern *c d e f*, filled with cold water constantly renewed. The pipe is then continued nearly horizontally along this cistern, and terminates at R in a pump T. The piston S has clack-valves opening upwards, and its rod S *s*, passing through a collar of hemp at T, is suspended by a chain to a small arch-head on the outer arm of the beam. There is a valve R in the bottom of this pump, as usual, which opens when pressed in the direction QR, and shuts against a contrary pressure. This pump delivers its contents into another pump XY, by means of the flat pipe *t* X, which proceeds from its top. This second pump has a valve at X, and a clack in its piston Z, as usual, and the piston-rod Z *z* is suspended from another arch-head on the outer arm of the beam. The two valves I and L are opened and shut by means of spanners and handles, which are put in motion by a plug-frame, in the same manner as in Newcomen's engine.

Lastly, there may be observed a crooked pipe *a b o*, which enters the upright pipe laterally a little above Q. This has a small jet hole at *o*; and the other end *a*, which is considerably under the surface of the water of the condensing cistern, is covered with a puppet-valve *v*, whose long stalk *v u* rises above the water, and may be raised or lowered by hand or by the plug-beam. The valves R and X, and the clacks in the pistons S and Z, are opened or shut by the pressures to which they are immediately exposed.

This figure is not an exact copy of any of Mr Watt's engines, but has its parts so disposed that all may come distinctly into view, and exactly perform their various functions. It is drawn in its quiescent position, the outer end of the beam preponderating by the counter-weight, and the piston P at the top of the cylinder, and the pistons S and Z in their lowest situations.

In this situation let us suppose that a vacuum is (by any means) produced in all the space below the piston, the valve I being shut. It is evident that the valve R will also be shut, as also the valve *v*. Now let the valve I be opened. The steam from the boiler, as elastic as common air, will rush into the space above the piston, and will exert on it a pressure as great as that of the atmosphere. It will therefore press it down, raise the outer end of the beam, and cause it to perform the same work as an ordinary engine.

When the piston P has reached the bottom of the cylinder, the plug-frame shuts the valve I, and opens L. By so doing, the communication is open between the top and bottom of the cylinder, and nothing hinders the steam which is above the piston from going along the passage MLON. The piston is now equally affected on both sides by the steam, even though a part of it is condensed in the pipe IOQR. Nothing therefore hinders the piston from being dragged up by the counter-weight, which acts with its whole force, undiminished by any remaining unbalanced elasticity of steam. Here therefore this form of the engine has an advantage (and by no means a small one) over the common engines, in which a great part of the counter-weight is expended in overcoming unbalanced atmospheric pressure.

Whenever the piston P arrives at the top of the cylinder, the valve L is shut by the plug-frame, and the valves I and *v* are opened. All the space below the piston is at this time occupied by the steam which came from the upper part of the cylinder, *by this time of greatly diminished elasticity, and not near* a balance for the pressure of the atmosphere. Therefore, during the ascent of the piston, the valve R was shut, and it remains so. When, therefore, the valve *v* is opened, the cold water of the cistern must spout up through the hole *o*, and condense the remaining steam. To this must be added the coldness of the whole pipe OQS. As fast as it is condensed, its place is supplied by steam from the lower part of the cylinder. We have already remarked, that this successive condensation is accomplished with astonishing rapidity. In the mean time, steam from the boiler presses on the upper surface of the piston. It must therefore descend as before, and the engine must perform a second working-stroke.

But, in the mean time, the injection-water lies in the bottom of the pipe OQR, heated to a considerable degree by the condensation of the steam; also a quantity of air has been disengaged from it, and from the water in the boiler. How is this to be discharged? This is the office of the pumps ST and XY. The capacity of ST is great in proportion to the space in which the air and water are lodged. When, therefore, the piston S has got to the top of its course, there must be a vacuum in the barrel of this pump, and the water and air must open the valve R and come into it. When the piston S comes down again in the next returning-stroke, this water and air get through the valve of the piston; and next, in the working-stroke, they are discharged by the piston into the pump XY, and raised by its piston in the following stroke. The air escapes at Y, and as much of the water as is necessary is delivered into the boiler by a small pipe Y *g* to supply its waste. It is a matter of indifference whether the pistons S and Z rise with the outer or inner end of the beam, but it is rather better that they rise with the inner end. They are otherwise drawn here in order to detach them from the rest, and show them more distinctly. *The steam-case is omitted in this drawing, but one method of applying it is represented in plate III.*

Such is Mr Watt's second engine. Let us examine its principles, that we may see the causes of its avowed and great superiority over the common engines.

59. We have already seen one ground of superiority, the full operation of the counter-weight. We are authorised, by careful examination, to say, that in the common

engines, at least one-half of the counter-weight is expended in counteracting an unbalanced pressure of the air on the piston during its ascent. In many engines, which are not the worst, this extends to one-fifth of the whole pressure. This is evident from the examination of the engine at Montrelaix, by Bossut. This makes a very great counter-weight necessary, which exhausts a proportional part of the moving force.

60. But the great advantage of Mr Watt's form is the almost total annihilation of the waste of steam by condensation in the cylinder. The cylinder is always boiling hot, and therefore perfectly dry. This must be evident to any person who understands the subject. By the time that Mr Watt had completed these improvements, his experiments on the production of steam had given him a pretty accurate knowledge of its density; and he found himself authorised to say, that the quantity of steam employed did not *much exceed what would fill the cylinder, so that very little* was unavoidably wasted. But before he could bring the engine to this degree of perfection, he had many difficulties to overcome: He inclosed the cylinder in *another containing steam, and that in a wooden case at a small distance from it, which effectually prevents all condensation in the* inner *cylinder from external influence; and the condensation by the outer cylinder itself, which was very small, had no other bad consequence than the loss of so much steam as formed the condensed water.* (*See Mr W.'s short history of the invention, p.* 117.)

The greatest difficulty was to make the great piston tight. The old and effectual method, by water lying on it, was inadmissible. He was therefore obliged to have his cylinders most nicely bored, perfectly cylindrical, and finely polished; and he made numberless trials of different soft substances for packing his piston, which should be tight without enormous friction, and which should long remain so, in a situation perfectly dry and very hot.

After all that Mr Watt has done in this respect, he thinks that the greatest part of the waste of steam which he still perceives in his engines, arises from the unavoidable escape by the sides of the piston during its descent.

But the fact is, that an engine of this construction, of the same dimensions with a common engine, making the same number of strokes of the same extent, does not consume above one-fourth *or one-third* part of the fuel that is consumed by the best engines of the common form. It is also a very fortunate circumstance, that the performance of the engine is not immediately destroyed, nor indeed sensibly diminished, by a small want of tightness in the piston. In the common engine, if air get in in this way, it immediately puts a stop to the work; but although even a considerable quantity of steam get past the piston during its descent, the rapidity of condensation is such, that hardly any diminution of pressure can be observed.

61. *When Newcomen's engines are working under loads inferior to their whole power, they are regulated to prevent shocks which would be prejudicial, by lessening the quantity of injection, or by shutting the injection-cock sooner: These new engines may, in some degree, be regulated in the same manner; but it is done more effectually and economically, first, by limiting the opening of the regulating-valve which admits the steam above the piston, and letting it continue so open during the whole length of the stroke; secondly, by letting it open fully at first, and shutting it completely when the piston has proceeded downwards only part of its stroke; or, lastly, by the use of a throttle-valve, which, acting in the same manner as the flood-gate of a mill, admits no more steam than gives the desired power. (See description of throttle-valve, p.* 154.)

62. *The second of these methods of regulating the power of the engine, forms the basis of what is called the* Expansive Engine, *which renders available the greater part of the power with which the steam would rush into empty space, were the piston acted upon by the whole force of the steam, from the bottom to the top of the stroke, through the whole length of the cy-*

linder, a principle which had first occurred to Mr Watt in 1769, *and was adopted in an engine at Soho manufactory, and some others, about* 1776, *and in* 1778 *at Shadwell waterworks, and afterwards particularly described in his specification of a patent for several new improvements upon steam-engines, in* 1782.

The construction of this engine is what has been described. The steam-valve I *is always allowed to open fully; the pins of the plug-frame are regulated so that that valve shall shut the moment that the piston has descended a certain portion, suppose one-fourth, one-third, or one-half of the length of the cylinder.* So far the cylinder was occupied by steam as elastic as common air. In pressing the piston farther down, it behoved the steam to expand, and its elasticity to diminish. It is plain that this can be done in any degree we please, and that the adjustment can be varied in a minute, according to the exigency of the case, by moving the plug-pins.

In the mean time, it must be observed, that the pressure on the piston is continually changing, and consequently the accelerating force. The motion, therefore, will no longer be uniformly accelerated: It will approach much faster to uniformity; nay, it may be retarded, because although the pressure on the piston at the beginning of the stroke may exceed the resistance of the load, yet when the piston is near the bottom, the resistance may exceed the pressure. Whatever may be the law by which the pressure on the piston varies, an ingenious mechanic may contrive the connecting machinery in such a way that the chains or rods at the outer end of the beam shall continually exert the same pressure, or shall vary their pressure according to any law he finds most convenient. It is in this manner that the watchmaker, by the form of the fuzee, produces an equal pressure on the wheel-work by means of a very unequal action of the main-spring. In like manner, by making the outer arch-heads portions of a proper spiral instead of a circle, we can regulate the force of the beam at pleasure.

Thus we see how much more manageable an engine is in this form than Newcomen's was, and also more easily investigated in respect of its power in its various positions. The knowledge of this last circumstance was of mighty consequence, and without it no notion could be formed of what it could perform, which may be called a discovery of great importance in the theory of the engine. *We shall give here Mr Watt's theory of the expansive engine which we have just described.*

63. Let ABCD (Plate II. fig. 10.) represent a section of the cylinder of a steam-engine, and EF the surface of its piston. Let us suppose that the steam was admitted while EF was in contact with AB, and that as soon as it had pressed it down to the situation EF, the steam-cock is shut. The steam will continue to press it down, and as the steam expands, its pressure diminishes. We may express its pressure (exerted all the while the piston moves from the situation AB to the situation EF) by the line EF. If we suppose the elasticity of the steam proportional to its density, as is nearly the case with air, we may express the pressure on the piston in any other position, such as KL or DC, by K *l* and D *c*, the ordinates of a rectangular hyperbola F *l c*, of which AE, AB are the assymptotes, and A the centre. The accumulated pressure during the motion of the piston from EF to DC, will be expressed by the area EF *c* DE, and the pressure during the whole motion by the area ABF *c* D A.

Now it is well known that the area EF *c* DE is equal to ABFE multiplied by the hyperbolic logarithm of $\frac{\text{AD}}{\text{AE}}$, $= \text{L}.\frac{\text{AD}}{\text{AE}}$, and the whole area ABF *c* DA is $=$ ABFE $\times$ $\left(1+\text{L}.\frac{\text{AD}}{\text{AE}}\right)$.

Thus let the diameter of the piston be 24 inches, and the pressure of the atmosphere on a square inch be 14 pounds; the pressure on the piston is 6333 pounds. Let the whole stroke be 6 feet, and let the steam be stopped when the piston has descended 18 inches, or 1.5 feet. The hyperbolic logarithm of $\frac{6}{1.5}$ is 1.3862943. Therefore the accumulated pressure ABF *c* DA is $= 6333 \times 2.3682943$, $= 15114$ pounds.

As few professional engineers are possessed of a table of hyperbolic logarithms, while tables of common logarithms are, or should be, in the hands of every person who is much engaged in mechanical calculations, let the following method be practised. Take the common logarithm of $\frac{AD}{AE}$, and multiply it by 2.3026; the product is the hyperbolic logarithm of $\frac{AD}{AE}$.

The accumulated pressure while the piston moves from AB to EF is 6333×1, or simply 6333 pounds. Therefore the steam while it expands into the whole cylinder adds a pressure of 8781 pounds.

Suppose that the steam had got free admission during the whole descent of the piston, the accumulated pressure would have been 6333×4, or 25332 pounds.

Here Mr Watt observed a remarkable result. The steam expended in this case would have been four times greater than when it was stopped at one-fourth, and yet the accumulated pressure is not twice as great, being nearly five-thirds. One-fourth of the steam performs nearly three-fifths of the work, and an equal quantity performs more than twice as much work when thus admitted during one-fourth of the motion.

This is curious and important information, and the advantage of this method of working a steam-engine increases in proportion as the steam is sooner stopped; but the increase is not great after the steam is rarefied four times. The curve approaches near to the axis, and small additions are made to the area. The expense of such great cylinders is considerable, and may sometimes compensate this advantage.*

Let the steam be stopped at	Its performance is mult.
$\frac{1}{2}$	1.7
$\frac{1}{3}$	2.1
$\frac{1}{4}$	2.4
$\frac{1}{5}$	2.6
$\frac{1}{6}$	2.8
$\frac{1}{7}$	3.
$\frac{1}{8}$	3.2
&c.	&c.

It is very pleasing to observe so many unlooked-for advantages resulting from an improvement made with the sole view of lessening the waste of steam by condensation. While this purpose is gained, we learn how to husband the steam which is not thus wasted. The engine becomes more manageable, and is more easily adapted to every variation in its task, and all its powers are more easily computed.

The active mind of its ingenious inventor did not stop here: It had always been matter of regret that one-half of the motion was unaccompanied by any work. It was a very obvious thing to Mr Watt, that as the steam admitted above the piston pressed it down, so steam admitted below the piston would press it up with the same force, provided that a vacuum were made on its upper side. This was easily done, by connecting the lower end of the cylinder with the boiler and the upper end with the condenser.

68. *Description of Mr Watt's Steam-Engine in its most improved State, called the Double Engine.*—Plate III. fig. 11. is a representation of this construction exactly copied

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* "All these calculations, however, proceed upon the supposition that steam contracts and expands by variations of *pressure*, in the same ratios that air would do." W.

from Mr Watt's figure accompanying his specification. Here BB is a section of the cylinder, surrounded at a small distance by the steam-case 1111. The section of the piston A, and the collar which embraces the piston-rod, gives a distinct notion of its construction, of the manner in which it is connected with the piston-rod, and how the packing of the piston and collar contributes to make all tight.

From the top of the cylinder proceeds the horizontal pipe. Above the letter D is observed the seat of the steam-valve, communicating with the box above it. In the middle of this may be observed a dark-shaded circle. This is the mouth of the upper branch of the steam-pipe coming from the boiler. Beyond D, below the letter N, is the seat of the upper condensing-valve. The bottom of the cylinder is made spherical, fitting the piston, so that they may come nearly into entire contact. Another horizontal pipe proceeds from this bottom. Above the letter E is the seat of the lower steam-valve, opening into the valve-box. This box is at the extremity of another steam-pipe marked C, which branches off from the upper horizontal part, and descends obliquely, coming forward to the eye. (See the front view, Plate III. fig. 12.) The lower part is represented as cut open, to shew its interior conformation. Beyond this steam-valve, and below the letter F, may be observed the seat of the lower condensing-valve. A pipe descends from hence, and at a small distance below unites with another pipe GG, which comes down from the upper condensing valve N. These two eduction-pipes thus united go downwards, and open at L into a rectangular box, of which the end is seen at L. This box goes backward from the eye, and at its farther extremity communicates with the air-pump K, whose piston is here represented in section with its butterfly valves. The piston delivers the water and air laterally into another rectangular box M, darkly shaded, which box communicates with the pump I. The piston-rods of this and of the air-pump are suspended by chains from a small arch-head on the inner arm of the great beam. The lower part of the eduction-pipe, the horizontal box L, the air-pump K, with the communicating box M between it and the pump I, are all immersed in the cold water of the condensing cistern. The box L is made flat, broad, and shallow, in order to increase its surface and accelerate the condensation. But that this may be performed with the greatest expedition, a small pipe H, open below (but occasionally stopped by a plug-valve,) is inserted laterally into the eduction-pipe G, and then divides into two branches, one of which reaches within a foot or two of the upper valve N, and the other approaches as near to the valve F.

As it is intended by this construction to give the piston a strong impulse in both directions, it will not be proper to suspend its rod by a chain from the great beam; for it must not only pull down that end of the beam, but also push it upwards. It may indeed be suspended by double chains, like the pistons of the engines for extinguishing fires; and Mr Watt has accordingly done so in some of his engines. But in his drawing, from which this figure is copied, he has communicated the force of the piston to the beam by means of a toothed rack OO, which engages or works in the toothed sector QQ on the end of the beam. The reader will understand, without any farther explanation, how the impulse given to the piston in either direction is thus transmitted to the beam without diminution.* The fly XX, with its pinion Y, which also works in the toothed arch QQ, may be supposed to be removed for the present, and will be considered afterwards.

We shall take the present opportunity of describing Mr Watt's method of communicating the force of the steam-engine to any machine of the rotatory kind. VV represents the rim and arms of a very large and heavy metalline fly. On its axis is the concentric-toothed wheel U. There is attached to the end of the great beam a strong and stiff rod TT, to the lower end of which a toothed wheel W is firmly fixed by two bolts, so that it cannot turn round. This wheel is of the same size and in the same vertical plane with the wheel U; and an iron link or strap (which cannot be seen here, because it is on the other side of the two wheels) connects the centres of the two wheels, so that the one cannot quit the other. The engine being in the position represented in the figure, suppose the fly to be turned once round by any external force in the direction of the darts. It is plain, that since the toothed wheels cannot quit each other, being kept together by the link, the inner half (that is, the half next the cylinder) of the wheel U will work on the inner half of the wheel W, so that at the end of the revolution of the fly the wheel W must have got to the top of the wheel U, and the outer end of the beam must be raised to its highest position. The next revolution of the fly will bring the wheel W and the beam connected with it to their first positions; and thus every two revolutions of the fly will make a complete period of the beam's reciprocating movements. Now, instead of supposing the fly to drive the beam, let the beam drive the fly. The motions must be perfectly the same, and the ascent or descent of the piston will produce one revolution of the fly.*

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* "These racks and sectors were very soon laid aside, and a better method, called the parallel motion (hereinafter described) employed instead of them." W.

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* "It is proper here to give the history of this invention. I had very early turned my mind to the producing continued motions round an axis, and it will be seen by reference to my first specification, in 1769, that I there described a steam-wheel, moved by the force of steam acting in a circular channel against a valve on one side, and against a column of mercury or some other fluid metal on the other side. This was executed upon a scale of about six feet diameter at Soho, and worked repeatedly, but was given up, as several practical objections were found to operate against it. Similar objections lay against other rotative engines which had been contrived by myself and others, as well as to the engines producing rotatory motions by means of

A front view of this double engine is given in fig. 12. marked by the same letters of reference. This shows the

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ratchet-wheels, mentioned § 47. Having made my reciprocating engines very regular in their movements, I considered how to produce rotative motions from them in the best manner; and amongst various schemes which were subjected to trial, or which passed through my mind, none appeared so likely to answer the purpose as the application of the crank in the manner of the common turning lathe, (an invention of great merit, of which the humble inventor, and even its æra, are unknown). But, as the rotative motion is produced in that machine by the impulse given to the crank in the descent of the foot only. and behoves to be continued in its ascent by the momentum of the wheel which acts as a fly, and being unwilling to load my engine with a fly heavy enough to continue the motion during the ascent of the piston, (and even were a counter-weight employed to act during that ascent, of a fly heavy enough to equalize the motion), I proposed to employ two engines acting upon two cranks fixed on the same axis at an angle of 120 degrees to one another, and a weight placed upon the circumference of the fly at the same angle to each of the cranks, by which means the motion might be rendered nearly equal, and a very light fly would only be requisite. This had occurred to me very early, but my attention being fully employed in making and erecting engines for raising water, it remained in petto until about the year 1778 or 9, when Mr Wasbrough erected one of his ratchet-wheel engines at Birmingham, the frequent breakages and irregularities of which recalled the subject to my mind, and I proceeded to make a model of my method, which answered my expectations; but having neglected to take out a patent, the invention was communicated by a workman employed to make the model to some of the people about Mr Wasbrough's engine, and a patent was taken out by them for the application of the crank to steam-engines. This fact the said workman confessed, and the engineer who directed the works acknowledged it, but said, nevertheless, the same idea had occurred to him prior to his hearing of mine, and that he had even made a model of it before that time, which might be a fact, as the application to a single crank was sufficiently obvious. In these circumstances I thought it better to endeavour to accomplish the same end by other means, than to enter into litigation, and, if successful, by demolishing the patent, to lay the matter open to every body. Accordingly, in 1781, I invented and took out a patent for several methods of producing rotative motions from reciprocating ones, amongst which was the method of the sun and planet wheels described in the text.

" This contrivance was applied to many engines, and possesses the great advantage of giving a double velocity to the fly; but is perhaps more subject to wear, and to be broken under great strains, than the crank, which is situation of parts which were fore-shortened in fig. 11., particularly the descending branch C of the steam-pipe, and the situation and communications of the two pumps K and I. 8, 8 is the horizontal part of the steam-pipe. 9 is a part of it, whose box is represented by the dark circle of fig. 11. D is the box of the steam-clack; and the little circle at its corner represents the end of the axis which turns it, as will be described afterwards. N is the place of the upper eduction-valve. A part only of the upper eduction-pipe G is represented, the rest being cut off, because it would have covered the descending steam-pipe CC. When continued down, it comes between the eye and the box E of the lower steam-valve, and the box F of the lower eduction-valve.

Let us now trace the operation of this machine through all its steps. Recurring to fig. 11., let us suppose that the lower part of the cylinder BB is exhausted of all elastic fluids; that the upper steam-valve D and the lower eduction-valve F are open, and that the lower steam-valve E and upper eduction-valve N are shut. It is evident that the piston must be pressed toward the bottom of the cylinder, and must pull down the end of the working-beam by means of the toothed rack OO and sector QQ, causing the other end of the beam to urge forward the machinery with which it is connected. When the piston arrives at the bottom of the cylinder, the valves D and F are shut by the plug-frame, and E and N are opened. By this last passage the steam gets into the eduction-pipe, where it meets with the injection water, and is rapidly condensed. The steam

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now more commonly used, although it requires a fly-wheel of four times the weight, if fixed upon the first axis. My application of the double engine to these rotative machines rendered unnecessary the counter weight, and produced a more regular motion; *so that, in most of our great manufactories, these engines now supply the place of water, wind, and horse mills; and instead of carrying the work to the power, the prime agent is placed wherever it is most convenient to the manufacturer.*" W.

from the boiler enters at the same time by E, and pressing on the lower side of the piston, forces it upwards, and by means of the toothed rack OO and toothed sector QQ forces up that end of the working-beam, and causes the other end to urge forward the machinery with which it is connected: and in this manner the operation of the engine may be continued for ever.

The injection water is continually running into the eduction-pipe, because condensation is continually going on, and therefore there is a continual atmospheric pressure to produce a jet. The air which is disengaged from the water, or enters by leaks, is evacuated only during the rise of the piston of the air-pump K.

It is evident that this form of the engine, by maintaining an almost constant and uninterrupted impulsion, is much fitter for driving any machinery of continued motion than any of the former engines, which were inactive during half of their motion. It does not, however, seem to have this superiority when employed to draw water; but it is also fitted for this task. Let the engine be loaded with twice as much as would be proper for it if a single-stroke engine, and let a fly be connected with it. Then it is plain that the power of the engine during the rise of the steam-piston will be accumulated in the fly; and this, in conjunction with the power of the engine during the descent of the steam-piston, will be equal to the whole load of water.*

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* "The engraving here referred to is copied from the drawing of the double engine in the above patent of 1782, and is that of an experimental engine, no others having ever been made exactly similar. I have now added engravings of one of the Albion-Mill engines, being one of the earliest double engines erected for sale. I do not exactly recollect the date of the invention of the double engine, but a drawing of it is still in my possession, which was produced in the House of Commons when I was soliciting the act of parliament for the prolongation of my patent in 1774-5. Having encountered much difficulty in teaching others the construction and use of the single en-

In speaking of the steam and eduction-valves, we said that they were all puppet-valves. Mr Watt at first employ-

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gine, and in overcoming prejudices, I proceeded no farther in it at that time, nor until, finding myself beset with an host of plagiaries and pirates in 1782, I thought it proper to insert it, and some other things, in the patent above-mentioned.

" The mention of the Albion mills induces me to say a few words respecting an establishment so unjustly calumniated in its day, and the premature destruction of which by fire, in 1791, was not improbably imputed to design. So far from being, as misrepresented, a monopoly injurious to the public, it was the means of considerably reducing the price of flour while it continued at work.

" It consisted of two engines, each of fifty horses power, and 20 pairs of millstones, of which twelve or more pairs, with the requisite machinery for dressing the flour and for other purposes, were generally kept at work. In place of wooden wheels, always subject to frequent derangement, wheels of cast-iron, with the teeth truly formed and finished, and properly proportioned to the work, were here employed, and other machinery, which used to be made of wood, was made of cast-iron, in improved forms; and I believe the work executed here may be said to form the commencement of that system of mill-work which has proved so useful to this country.

" In the construction of that mill-work and machinery, Boulton and Watt derived most valuable assistance from that able mechanician and engineer, Mr John Rennie, then just entering into business, who assisted in planning them, and under whose direction they were executed.

" The engines and mill-work were contained in a commodious and elegant building, designed and executed under the direction of the late Mr Samuel Wyatt, architect.

" Though the double engines have been principally applied to rotative motions, yet where mines are very deep, they are advantageously applied to the working of pumps by a reciprocating motion; one set or half of the pump-rods being suspended by means of a sloping rod from the working-beam near the cylinder, and the other half of these rods being suspended directly from the outer end of that beam, so that the ascending motion of the piston pulls up one half of these rods, and works the pumps belonging to them, and the descending motion of the piston pulls up the other half of the rods, and works their pumps.

" An engine of this construction was erected at Wheal Maid Mine, in Cornwall, in the year 1787, or beginning of 1788, having a cylinder of sixty-three inches diameter, and nine feet stroke; but the stroke in the pumps, which were eighteen inches diameter, was only seven feet.

" This engine, which at the time it was made, was the most powerful in the ed cocks, and also sliding-valves, such as the regulator or steam-valves in the old engines. But he found them always lose their tightness after a short time. This is not surprising, when we consider that they are always perfectly dry, and very hot. He was therefore obliged to change them all for the puppet-clacks, which, when truly ground and nicely fitted in their motions at first, are not found to go soon out of order. Other engineers now universally use them in the old form of the steam-engine, without the same reasons, and merely by servile and ignorant imitation.

The way in which Mr Watt opens and shuts these valves is as follows. Fig. 13 represents a clack with its seat and box. Suppose it one of the eduction-valves. HH is part of the pipe which introduces the steam, and GG is the upper part of the pipe which communicates with the condenser. At EE may be observed a piece more deeply shaded than the surrounding parts. This is the seat of the valve, and is a brass or bell-metal ring turned conical on the outside, so as to fit exactly into a conical part of the pipe GG. These two pieces are fitted by grinding; and the cone being of a long taper, the ring sticks firmly in it, especially after having been there for some time and united by rust. The clack itself is a strong brass plate D, turned conical on the edge, so as to fit the conical or sloping inner edge of the seat. These are very nicely ground on each other with emery. This conical joining is much more obtuse than the outer side of the ring; so that although the joint is air-tight, the two pieces do not stick strongly together. The clack has a round tail DL, which is freely moveable up and

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world, worked remarkably well, though, like many others in Cornwall, it was loaded with an enormous weight of dry pump-rods.

"In other cases, when it has not been convenient to divide the pump-rods into two sets, the ascending motion of the piston has been employed to raise a weight equal to one-half the column of water in the pumps, which weight came in aid of the power of the engine in the descending stroke of the piston; but the former method is preferable, wherever it can be adopted." W.

down in the hole of a cross piece FF. On the upper side of the valve is a strong piece of metal DC, firmly joined to it, one side of which is formed into a toothed rack. A is the section of an iron axle which turns in holes in the opposite sides of the valve-box, where it is nicely fitted by grinding, so as to be air-tight. One end of this axis projects a good way without the box, and carries a spanner or handle, which is moved by the plug-frame. To this axis is fixed a strong piece of metal B, the edge of which is formed into an arch of a circle having the axis A in its centre, and is cut into teeth, which work in the teeth of the rack DC. K is a cover which is fixed by screws to the top of the box HJJH, and may be taken off in order to get at the valve when it needs repairs.

From this description it is easy to see, that by turning the handle which is on the axis A, the sector B must lift up the valve by means of its toothed rack DC, till the upper end of the rack touch the knob or button K. Turning the handle in the opposite direction brings the valve down again to its seat.

This valve is extremely tight. But in order to open it for the passage of the steam, we must exert a force equal to the pressure of the atmosphere. This, in a large engine, is a very great weight. A valve of six inches diameter sustains a pressure not less than 400 pounds. But this force is quite momentary, and hardly impedes the motion of the engine; for the instant the valve is detached from its seat, *and has moved through a very small space, the great force of the pressure is over.* Even this little inconvenience has been removed by a thought of Mr Watt. He has put the spanner in such a position when it *begins* to raise the valve, that its mechanical energy is almost infinitely great. Let QR (fig. 14.) be part of the plug-frame descending, and P one of its pins just going to lay hold of the spanner NO, moveable round the axis N. On the same axis is another arm NM, connected by a joint with the leader ML, which is connected also by a joint with the spanner LA that is on the axis A of the sector within the valve-box. Therefore, when the pin P pushes down the spanner NO, the arm NM moves sidewise and pulls down the spanner AL by means of the connecting rod. Things are so disposed, that when the cock is shut, LM and MN are in one straight line. The intelligent mechanic will perceive that, in this position, the force of the lever ONM is insuperable. It has this further advantage, that if any thing should tend to force open the valve, it would be ineffectual; for no force exerted at A, and transmitted by the rod LM, can possibly push the joint M out of its position. Of such importance is it to practical mechanics, that its professors should be persons of penetration as well as knowledge. Yet this circumstance is unheeded by hundreds who have servilely copied from Mr Watt, as may be seen in every engine that is puffed on the public as a discovery and an improvement. When these puppet-valves have been introduced into the common engine, we have not seen one instance where this has been attended to; certainly because its utility has not been observed.* (See also the description of the working-gear of the Albion-mill engines, and Plate VI.)

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* "This was the construction of the regulator boxes or nozles which was generally employed by Boulton and Watt about the time when this essay was written; but upon the introduction of the double engines, it was somewhat changed, the regulator or valve, which admitted the steam to the cylinder, was placed directly over the exhaustion-valve, which admitted the steam to the condenser, as may be seen in the drawing of one of the Albion-Mill engines, represented in Plate B.

"At a later period, my friend Mr Murdock contrived another arrangement, which is now generally followed in steam-engines applied to the working pumps, or other reciprocating motions: The regulating-valves are placed one over another, as has just been mentioned; the stems of the steam-regulators are hollow cylinders, through which, and a collar of hemp, the stems of the exhaustion-valves pass upwards air-tight, and these hollow stems of the steam-regulators also pass through stuffing-boxes in the covers of the regulator boxes. In this arrangement the regulators or valves are

We postponed any account of the office of the fly XX (fig. 11.), as it is not of use in an engine regulated by the fly VV. The fly XX is only for regulating the reciprocating motion of the beam when the steam is not admitted during the whole descent of the piston. This it evidently must render more uniform, accumulating a momentum equal to the whole pressure of the full supply of steam, and then sharing it with the beam during the rest of the descent of the piston.†

69. When a person properly skilled in mechanics and chemistry reviews these different forms of Mr Watt's steam-engine, he will easily perceive them susceptible of many intermediate forms, in which any one or more of the distinguishing improvements may be employed. The first great improvement was the condensation in a separate vessel. This increased the original powers of the engine, giving to the atmospheric pressure and to the counterweight their full energy; at the same time the waste of steam is greatly diminished. The next improvement, by employing the pressure of the steam instead of that of the atmosphere, aimed not only at a still farther diminution of the waste; but was also fertile in advantages, rendering

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raised and depressed by means of machinery fixed upon the outside (in place of that described in the inside), and are preferable from their being more easily accessible in case of derangement.

"Mr Murdock has also contrived an excellent sliding valve for the admission of steam into, and its exit from, the cylinder (now very generally used, with some improvements, by Boulton, Watt, and Co. in their rotative engines), for which, along with several other articles, he obtained his majesty's patent in 1799, (the specification of which has been published in the Repertory of Arts, vol. xiii.) To the ingenuity of Mr Murdock are also due many improvements in the manufacture of the engines, and in the machines and tools used for that purpose."

† "This has been found to be unnecessary; for even in pumping engines, the momentum of the great beam pump-rods, water, &c. is found to afford sufficient regulation for the inequality of the stroke, and in rotative engines the great fly performs that office." W.

the machine more manageable, and particularly enabling us at all times, and without trouble, to suit the power of the engine to its load of work, however variable and increasing; and brought into view a very interesting proposition in the mechanical theory of the engine, viz that the whole performance of a given quantity of steam may be augmented by admitting it into the cylinder only during a part of the piston's motion. Mr Watt has varied the application of this proposition in many ways; and there is nothing about the machine which gives more employment to the sagacity and judgment of the engineer. The third improvement of the double impulse may be considered as the finishing touch given to the engine, and renders it as uniform in its action as any water-wheel. In the engine in its most perfect form there does not seem to be above one-fourth of the steam wasted; so that *it is not possible* to make it one-fourth part more powerful than it is at present.

70. The only thing that seems susceptible of considerable improvement is the great beam. The enormous strains exerted on its arms require a proportional strength. This requires a vast mass of matter, not less indeed in an engine with a cylinder of 54 inches than three tons and a half, moving with the velocity of three feet in a second, which must be communicated in about half a second. This mass must be brought into motion from a state of rest, must again be brought to rest, again into motion, and again to rest, to complete the period of a stroke. This consumes much power; and Mr Watt has not been able to load an engine with more than 10 or 11 pounds on the inch and preserve a sufficient quantity of motion, so as to make 12 or 15 8-feet strokes in a minute.*

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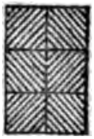
* "There is no loss of power except the friction, as the power employed to give motion in the beginning of the stroke is returned in the latter part of it, by continuing the motion after the steam-regulating valve is shut." W.



The difficulty of obtaining timber of sufficient dimensions to form the working-beam of a very powerful engine in one log, early suggested to the constructors of steam-engines the forming them of six or more logs laid flat upon one another, and side to side, (as shewn in this cross section,) and screwed together upon dowells or joggles to prevent them from sliding upon one another; but however well working-beams so constructed have been put together, it has been found that after they have been some time in use the straps and bolts by which the logs were connected have gradually got loose, and several bad consequences have ensued. Beams in one solid log were therefore preferred wherever they could be obtained. In small engines Mr Watt sometimes used cast-iron wheels, or large pulleys, in place of working-beams, and occasionally other contrivances.*

71. We presume that our thinking readers will not be displeased with this rational history of the progress of this engine in the hands of its ingenious and worthy inventor. We owe it to the communications of a friend, well acquainted with him, and able to judge of his merits. The

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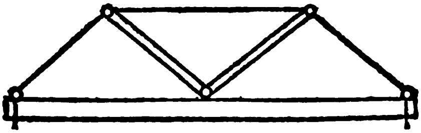
* "To prevent this, whenever timber could not be got large enough to form the beams in one piece, I latterly resorted to simple beams, braced with iron in this manner:



But for several years past, working-beams of timber for engines of every size have been entirely laid aside, and those made of cast-iron have been employed in place of them, whereby the bending, splitting, twisting, dry-rot, &c. to which wood is subject, are completely avoided." W.

public see him always associated with the no less celebrated mechanic and philosopher Mr Boulton, of Soho, near Birmingham. They have shared the royal patent from the beginning; and the alliance is equally honourable to both.*

72. The advantages derived from the patent-right show both the superiority of the engine and the liberal minds of the proprietors. They erect the engines at the expence of the employers, or give working drafts of all the parts, with instructions, by which any resident engineer may execute the work. The employers select *the best engine of the ordinary kind in the neighbourhood using the same sort of coals*, compare the quantities of fuel expended by each, and pay to Messrs Watt and Boulton one-third of the annual savings for a certain term of years. By this the patentees are excited to do their utmost to make the engine perfect; and the employer pays in proportion to the advantage he derives from it.†

It may not be here improper to state the actual performance of some of these engines.

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* "The late Dr Roebuck of Borrowstoness, a gentleman of much ingenuity and enterprise, was originally associated with me in the profits which might accrue from the patent, but in or about 1773 he disposed of his interest in it to Mr Boulton, and both of them, in 1774-5, assisted me in procuring an act of parliament for the prolongation of the patent for 25 years from that time; and I then commenced a partnership with the latter, which terminated with the exclusive privilege in the year 1800, when I retired from business, but our friendship continued undiminished to the close of his life. As a memorial due to that friendship, I avail myself of this, probably a last public opportunity, of stating, that to his friendly encouragement, to his partiality for scientific improvements, and his ready application of them to the processes of art; to his intimate knowledge of business and manufactures, and to his extended views and liberal spirit of enterprise, must in a great measure be ascribed whatever success may have attended my exertions." W.

† "This was originally their method of agreeing, but afterwards, to avoid disputes and trouble, they fixed certain rates for each sized engine, according to the value and quality of coals in the neighbourhood." W.

73. *The burning of one bushel of good Newcastle or Swansea coals in Mr Watt's reciprocating engines, working more or less expansively, was found, by the accounts kept at the Cornish mines, to raise from 24 to 32 millions of pounds of water one foot high: the greater or less effect depending upon the state of the engine, its size, and rate of working, and upon the quality of the coal.*

In engines upon the rotative double construction, one having a cylinder of 31½ inches diameter, and making 17½ strokes of 7 feet long per minute, called 40 horses' power,[*] meaning the constant exertion of 40 horses (for which purpose, supposing the work to go on night and day, 3 relays, or at least 120 horses, must be kept,) consumed about 4 bushels of good Newcastle coal per hour, or 400 weight of good Wednesbury coal. A rotative double engine, with a cylinder of 23¾ inches in diameter, making 21½ strokes of 5 feet long per minute, was called 20 horses' power; and an engine, with a cylinder of 17½ inches diameter, making 25 strokes of 4 feet long per minute, was called 10 horses' power; and the consumption of coals by these was nearly proportional to that of the 40 horses' power.

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[*] "When Boulton and Watt set about the introduction of the rotative steam-engines, to give motion to mill-work, they felt the necessity of adopting some mode of describing the power, which should be easily understood by the persons who were likely to use them. Horses being the power then generally employed to move the machinery in the great breweries and distilleries of the metropolis, where these engines came first into demand, the power of a mill-horse was considered by them to afford an obvious and concise standard of comparison, and one sufficiently definite for the purpose in view.

"A horse going at the rate of 2½ miles an hour raises a weight of 150 lbs. by a rope passing over a pulley, which is equal to the raising 33,000 pounds one foot high in a minute. This was considered the horse's power; but in calculating the size of the engines, it was judged advisable to make a very ample allowance for the probable case of their not being kept in the best order, and therefore the load was only assumed at about 7 lbs. on the square inch of the piston, although the engines work well to 10 lbs. on the inch, exclusive of their own friction." W.

A bushel of Newcastle coals, which thus appears to be the consumption of a 10-horse engine for one hour, grinds and dresses about 10 bushels, Winchester measure, of wheat.

The quantity of water necessary for injection may be determined on principle for engines having a separate condenser. Having found the contents of the cylinder in cubic feet (that is, the area of the piston multiplied by the length of the stroke $+\frac{1}{10}$*, to allow for the vacuities at top and bottom through which the piston does not pass), it is to be considered that every cubic foot of steam produces about a cubic inch of water when condensed, and contains about as much latent heat as would raise 960 cubic inches of water one degree. This steam must not only be condensed, but must be cooled down to the temperature of the hot well: therefore as many inches of cold water must be employed as will require all this heat to raise it to the temperature of the hot well.*

Therefore let c be the quantity of steam to be condensed in cubic feet;

a *= the temperature of the cold water (per Fahrenheit);*
b *= the proposed temperature of the warm water, or hot well;*
1172 *= the sensible and latent heat of steam;*
x *= the cubic inches of cold water required to condense c.*
Then $c \times \overline{1172-b} = x \times \overline{b+a}$*:*

Therefore $c \times \dfrac{\overline{1172-b}}{b-a} = x.$

Thus, if the proposed temperature of the hot well be 100° (and it should not be higher to obtain a tolerable vacuum in the cylinder), and that of the injection be 50°, we have $a = 50°$*,* $b = 100°$*; hence* $\dfrac{1172-100}{100-\ \ 50} = 21.44 = x$*. That is, for every foot of the capacity of the stroke in the cylinder* $+\frac{1}{10}$*, calculated as has been directed, or for every cubic inch of water evaporated from the boiler, about* $21\frac{1}{2}$ *cubic inches of water at 50° will be required to condense the steam.*

But as the injection water may not be obtained so cold as 50°, and other circumstances may require an allowance, a wine *pint of water for every inch boiled off, or for every cubic foot of the contents of the stroke in the cylinder, may be kept in mind as amply sufficient. This greatly exceeds the quantity necessary in a good Newcomen's engine, and by showing the more perfect condensation, points out the superiority of the new engine; for the Newcomen's engine, if working to the greatest advantage, should not be loaded to more than 7 pounds upon the inch, whereas Watt's engine bears a load not much less than* 11 *pounds, exclusive of friction, when making* 12 8-*feet strokes per minute.*

What has been now said is not a matter of mere curiosity: it affords an exact rule for judging of the good working order of the engine. We can measure with accuracy the water admitted into the boiler during an hour without allowing its surface to rise or fall, and also the water employed for injection. If the last be above the proportion now given (adapted to the temperatures 50° *and* 100°*), we are certain that steam is wasted by leaks, or by condensation in some improper place.*

It is evident that it is of great importance to have the temperature of the hot well as low as possible, because there always remains steam in the cylinder of the same or rather higher temperature, possessing an elasticity which balances part of the pressure on the other side of the piston, and thus diminishes the power of the engine. This is clearly seen by the barometer which Mr Watt applies to his engines, and is a most useful addition to the proprietor. It shows him the state of the vacuum, and, with the height of the mercury in the steam-guage, points out the real power of the engine.

Mr Watt finds that, with the most judiciously constructed furnaces, it requires 8 *feet of surface of the boiler to be exposed to the action of the fire and flame to boil off a cubic foot of water in an hour, and that a bushel of Newcastle coals so applied will boil off from* 8 *to* 12 *cubic feet, and that it requires about a cwt. of Wednesbury coals to do the same. W.*

In consequence of the great superiority of Mr Watt's engines, both with respect to economy and manageableness, they have become of most extensive use; and in every demand of manufacture on a great scale, they offer us an indefatigable servant, whose strength has no bounds.

74. The greatest mechanical project that ever engaged the attention of man was on the point of being executed by this machine. The States of Holland were treating with Messrs Watt and Boulton for draining the Haerlem Meer, and even reducing the Zuyder Zee: and we doubt not but that it will be accomplished whenever that unhappy nation has sufficiently felt the difference between liberty and foreign tyranny.* Indeed such unlimited powers are afforded by this engine, that the engineer now thinks that no task can be proposed to him which he cannot execute with profit to his employer.

75. No wonder then that all classes of engineers have turned much of their attention to this engine; and, seeing that it has done so much, that they try to make it do still more. Numberless attempts have been made to improve Mr Watt's engine; and it would occupy a volume to give an account of them, whilst that account would do no more than indulge curiosity. Our engineers by profession are in general miserably deficient in that accurate knowledge of mechanics and of chemistry, which is necessary for understanding this machine; and we have not heard of one in this kingdom who can be put on a par with the present patentees in this respect. Most of the attempts of engineers have been made with the humbler view of availing themselves of Mr Watt's discoveries, so as to construct a steam-engine superior to Newcomen's, and yet of a form sufficiently different from Watt's to keep it without the reach of his patent. This they have in general accomplished by performing the condensation in a place which, with a little

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* "Such things were in agitation, but did not come so far as treating. A considerable engine was, however, erected by them at Mydrecht for draining a large extent of country, in which it was successful." W.

stretch of fancy, not unfrequent in a court of law, may be called *part of the cylinder*.

76. The success of most of these attempts has interfered so little with the interest of the patentees, that they have not hindered the erection of many engines which the law would have deemed encroachments. We think it our duty to give our opinion on this subject without reserve. These are most expensive undertakings, and few employers are able to judge accurately of the merits of a project presented to them by an ingenious artist. They may see the practicability of the scheme, by having a general notion of the expansion and condensation of steam, and they may be misled by the ingenuity apparent in the construction. The engineer himself is frequently the dupe of his own ingenuity; and it is not always dishonesty, but frequently ignorance, which makes him prefer his own invention or (as he thinks it) improvement. It is a most delicate engine, and requires much knowledge to see what does and what does not improve its performance. We have gone into the preceding minute investigation of Mr Watt's progress, with the express purpose of making our readers fully masters of its principles, and have more than once pointed out the real improvements, that they may be firmly fixed and always ready in the mind. By having recourse to them, the reader may pronounce with confidence on the merits of any new construction, and will not be deceived by the puffs of an ignorant or dishonest engineer.* †

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* Dr Robison's account of Hornblower's engine has been omitted by the Editor, as unsuitable to the present work. Notwithstanding the great ingenuity evinced by Mr Hornblower in the construction of his engine, yet it was found by a court of law to be a plagiarism of Mr Watt's invention, and on this account Mr Watt could not have made any commentary upon it, even if it had appeared deserving of insertion. EDITOR.

† "Several of my improvements mentioned in the course of this essay were, with other contrivances which I have not thought it necessary to enter into

77. We shall conclude our account of this noble engine with observing, that Mr Watt's form suggests the con-

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the detail of, made the objects of distinct patents subsequent to that of 1769, of which the following notice may here suffice:

PATENT, 25th October 1781. "For certain new Methods of applying the Vibrating or Reciprocating Motion of Steam or Fire-Engines to produce a continued Rotative or Circular Motion round an Axis or Centre, and thereby to give Motion to the Wheels of Mills or other Machines."

The specification dated 13th February, 1782, contains a description of five different contrivances of rotative motions.

PATENT, 12th March 1782. "For certain new Improvements upon Steam or Fire-Engines for raising Water, and other Mechanical Purposes, and certain new Pieces of Mechanism applicable to the same"

The specification, dated the 3d July, 1782, contains, first, The expansive steam-engine, with six different contrivances for equalizing the power; second, the double-power steam-engine, in which the steam is alternately applied to press on each side of the piston, while a vacuum is formed on the other; third, a new compound engine, or method of connecting together the cylinders and condensers of two or more distinct engines, so as to make the steam which has been employed to press on the piston of the first, act expansively upon the piston of the second, &c., and thus derive an additional power to act either alternately or conjointly with that of the first cylinder; fourth, the application of toothed racks and sectors to the ends of the piston or pump-rods, and to the arches of the working-beams, instead of chains; fifth, a new reciprocating semi-rotative engine, and a new rotative engine or steam-wheel.

PATENT, 28th April, 1784. "For certain new Improvements upon Fire and Steam-Engines, and upon Machines worked or moved by the same."

The specification, dated the 24th August, 1784, describes, first, A new rotative engine, in which the steam-vessel turns upon a pivot, and is placed in a dense fluid, the resistance of which to the action of the steam causes the rotative motion; second, Methods of causing the piston-rods, pump-rods, and other parts of engines, to move in perpendicular or other straight lines, and to enable the engine to act upon the working-beams both in pushing and pulling: This is now called the *Parallel-Motion*, and three varieties are described; third, Improved methods of applying the steam engine to work pumps, or other alternating machinery, by making the rods balance each other; fourth, A new method of applying the power of steam-engines to move mills which have many wheels required to move round in concert; fifth, A simplified method of applying the power of steam-engines to the working of heavy hammers or stampers; sixth, A new construction and mode of opening the valves, and an improved working-gear; seventh, A portable steam-engine and machinery for moving wheel-carriages.

struction of an excellent air-pump. A large vessel may be made to communicate with a boiler at one side, and with the pump-receiver on the other, and also with a condenser. Suppose this vessel of ten times the capacity of the receiver; fill it with steam from the boiler, and drive out the air from it; then open its communication with the receiver and the condenser. This will rarefy the air of the receiver ten times. Repeating the operation will rarefy it 100 times; the third operation will rarefy it 1000 times; the fourth, 10,000 times, &c. All this may be done in half a minute.

PATENT in 1785. "For certain newly improved Methods of constructing Furnaces, or Fire Places for Heating, Boiling, or Evaporating of Water, and other Liquids, which are applicable to Steam-Engines and other purposes; and also for Heating, Melting, and Smelting of Metals and their Ores, whereby greater effects are produced from the Fuel, and the Smoke is in great measure prevented or consumed."

The specification is dated the 14th June, 1785. W.

APPENDIX.

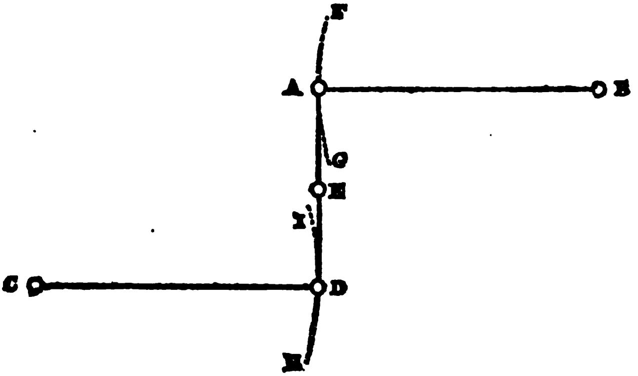
BY

MR WATT.

Dr Robison's account of Mr Watt's engines terminates here; but it seems necessary to describe and explain certain additions to them which he has either very slightly noticed, or not at all, though they existed before he composed the preceding dissertation; probably they were omitted from his not having had an opportunity of observing them closely.

allel
tion.

The first is the Parallel Motion, which, in the single engines, serves in place of chains, and in the double engines, supplies the place of the rack and sector, which has been described in art. 68. It has been mentioned, that the racks and sectors were very subject to wear, and that, when perfect, they did not move with that smoothness that was wished; and to chains there were many objections. It occurred to Mr Watt, that if some mechanism could be devised moving upon centres, which would keep the piston-rods perpendicular, both in pushing and pulling, that a smoother motion would be attained; and, in all probability, that the parts would be less subject to wear. After some consideration, it occurred, that if two levers of equal lengths were placed in the same vertical plane, nearly as shown in the following diagram, moveable on the centres B and C, and connected by a rod A D, the point E, in the middle of that rod, would describe nearly a straight and perpendicular line, when the ends A and D of the levers, and of that rod, moved in the segments of circles FG, and IH, provided the arch FG did not much exceed 40 degrees, and consequently that if the top of the piston-rod were attached to that point E, it would be guided perpendicularly, or nearly so.

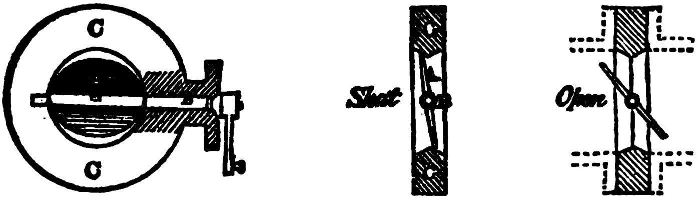


It necessarily followed, that if for convenience the lever CD (which represents what he called the regulating-radius) were made only half the length of the lever AB, (which represents the half length or radius of the working-beam) a point situated at one-third of the length of the rod AD, from the joint A, would then move in a perpendicular line. These were first ideas, but the parallel motion soon was moulded into the form in which it appears in all Boulton and Watt's engines, and in which it is seen in the annexed plate of the second engine at the Albion Mill. A patent for the protection of this, and some other of Mr Watt's inventions, passed the seals in April 1784, but the invention was made in the latter end of 1783.

A second article omitted to be described, is the method of regulating the speed of the rotative engines, a matter essential to their application to cotton-spinning, and many other manufactories.

Throttle-valve.

It is performed by admitting the steam into the cylinder more or less freely, by means of what is called a *Throttle-valve*, which is commonly a circular plate of metal A, having a spindle B fixed across its diameter.



This plate is accurately fitted to an aperture in a metal ring CC, of some thickness, through the edgeway of which the spindle is fitted steam-tight, and the ring is fixed between the two flanches of the joint of the steam-pipe which is next to the cylinder. One end of the spindle, which has a square upon it, comes through the ring, and has a spanner fixed upon it, by which it can be turned in either direction.

When the valve is parallel to the outsides of the ring, it shuts the opening nearly perfectly; but when its plane lies at an angle to the ring, it admits more or less steam according to the degree it has opened; consequently the piston is acted upon with more or less force. For many purposes engines are thus regulated by hand at the pleasure of the attendant; but where a regular velocity is required, other means must be applied to open and shut it, without any attention on the part of those who have the care of it. For this purpose Mr Watt had various methods, but at last fix-

Governo

ed upon what he calls the *Governor*, (shewn at W. Plate IV. and VII.) consisting of a perpendicular axis, turned by the engine: To a joint near the top of this axis are suspended two iron rods carrying heavy balls of metal at their lower ends, in the nature of pendulums. When this axis is put in motion by the engine, the balls recede from the perpendicular by the centrifugal force, and by means of a combination of levers fixed to their upper end, raise the end of a lever which acts upon the spanner of the throttle-valve, and shuts it more or less according to the speed of the engine, so that as the velocity augments, the valve is shut, until the speed of the engine and the opening of the valve come to a maximum and balance each other.

The application of the centrifugal *principle* was not a new invention, but had been applied by others to the regulation of water and wind-mills, and other things; but Mr Watt improved the mechanism by which it acted upon the machines, and adapted it to his engines.

Baromete
water an
steam-ga

From the beginning, Mr Watt applied a gage to shew the height of the water in his little boiler, which consisted of a glass tube communicating at the lower end with the water in the boiler, and at the upper end with the steam contained in it. This gage was of great use in his experiments, but in practice other methods are adopted. He has always used a barometer to indicate the degree of exhaustion in his engines. Sometimes that instrument is, as usual, a glass tube 33 or 34 inches long, immersed at bottom in a cistern of mercury, and at top communicating by means of a small pipe and cock with the condenser. The oscillations are in a great degree prevented by throttling the passage for the steam by means of the cock.

But as glass tubes were liable to be broken by the workmen, barometers were made of iron tubes, in the form of inverted syphons, one leg about half the length of the other, to the upper end of the long leg a pipe and cock were joined, which communicated with the condenser; a proper quantity of mercury was poured into the short leg of the syphon, which naturally stood level in the two legs: A light float with a slender stem was placed in the short leg, and a scale divided into half inches applied to it, which (as by the exhaustion the mercury rose as much in the long leg as it fell in the short one) represented inches on the common barometer.

am-gage.

The steam-gage is a short glass tube with its lower end immersed in a cistern of mercury, which is placed within an iron box screwed to the boiler steam-pipe, or to some other part communicating freely with the steam, which, pressing on the surface of the mercury in the cistern, raises the mercury in the tube, (which is open to the air at the upper end) and its altitude serves to show the elastic power of the steam over that of the atmosphere.

These instruments are of great use where they are kept in order, in shewing the superintendent the state of the engine; but slovenly engine-tenders are but too apt to put them out of order, or to suffer them to be so. *It is the interest, however, of every owner of an engine to see that they, as well as all other parts of the engine, are kept in order.*

e indica-

The barometer being adapted only to ascertain the degree of exhaustion in the condenser where its variations were small, the vibrations of the mercury rendered it very difficult, if not impracticable, to ascertain the state of the exhaustion of the cylinder at the different periods of the stroke of the engine; it became therefore necessary to contrive an instrument for that purpose that should be less subject to vibration, and should show nearly the degree of exhaustion in the cylinder at all periods. The following instrument, called the *Indicator*, is found to answer the end sufficiently. A cylinder about an inch diameter, and six inches long, exceedingly truly bored, has a solid piston accurately fitted to it, so as to slide easy by the help of some oil; the stem of the piston is guided in the direction of the axis of the cylinder, so that it may not be subject to jam or cause friction in any part of its motion. The bottom of this cylinder has a cock and small pipe joined to it, which, having a conical end, may be inserted in a hole drilled in the cylinder of the engine near one of the ends, so that by opening the small cock, a communication may be effected between the inside of the cylinder and the indicator.

The cylinder of the indicator is fastened upon a wooden or metal frame, more than twice its own length; one end of a spiral steel spring, like that of a spring steelyard, is attached to the upper part of the frame, and the other end of the spring is attached to the upper end of the piston-rod of the indicator. The spring is made of such a strength, that when the cylinder of the indicator is perfectly exhausted, the pressure of the atmosphere may force its piston down within an inch of its bottom. An index being fixed to the top of its piston-rod, the point where it stands, when quite exhausted, is marked from an observation of a barometer communicating with the same exhausted vessel, and the scale divided accordingly.

Piston-grease.

Mr Watt very early found that, although most kinds of grease would answer when employed to keep the piston tight, yet that beef or mutton tallow were the most proper, and the least liable to decompose; but when cylinders were new and imperfectly bored, the grease soon disappeared, and the piston was left dry; he therefore endeavoured to detain it by thickening it with some substance which would lubricate the cylinder, and not prove decomposable by heat and exhaustion. Black-lead dust seemed a proper substance, and was therefore employed, especially when a cylinder or the packing of the piston was new; but it was found in the sequel that the black-lead wore the cylinder, though slowly; and by more perfect workmanship, cylinders are made so true as not to require it, or at least only for a very short time at first using.

Cemen for the joints.

The joints of the cylinder, and other parts of Newcomen's engines, were generally made tight by being screwed together upon rings of lead covered with glaziers' putty, which method was sufficient, as the entry of small quantities of air did not materially affect the working of these engines where only a very imperfect exhaustion was required. But the contrary being the case in the improved engines, this method would not answer Mr Watt's purpose. He at first made his joints very true, and screwed them together upon pasteboard, softened by soaking in water, which answered tolerably well for a time, but was not sufficiently durable. He therefore endeavoured to find out some more lasting substance; and observing that at the iron founderies they filled up flaws by iron borings or filings, moistened by urine, which in time became hard, he improved upon this by mixing the iron borings or filings with a small quantity of sulphur, and a little salammoniac, to which he afterwards added some fine sand from the grindstone troughs. This mixture, being moistened with water and spread upon the joint, heats soon after it is screwed together, becomes hard, and remains good and tight for years, which has contributed in no small degree to the perfection of the engines.

Mr Murdock, much about the same time, without communication with Mr Watt, made a cement of iron borings and salammoniac, without the sulphur. But the latter gives the valuable property of making the cement set immediately.

The act of parliament extending Mr Watt's exclusive privilege for the improvements secured to him by his first patent, expired in 1800, at which period he retired from business, having for some years before ceased to take an active part. Previous to that time, Messrs Boulton and Watt, juniors, had been received into the partnership, and had erected extensive works, with improved machinery, for the manufacture of steam-engines; and, since the death of Mr Boulton, the concern has been carried on by them, in conjunction with Mr Southern and Mr Murdock. Under their management, considerable improvements have been made in the execution of the engines, and some advantageous alterations have been adopted in the subordinate mechanism and general construction; the use of wood has been discontinued, and iron, or, in the fixed parts, stone, or brick-work, substituted in its place; and numbers of expert workmen have been trained, so that the engines are now executed with a degree of perfection equalling, if not surpassing, the improvements which the increased wealth and commerce of the country have called forth in almost every other art and manufacture.

LETTER

FROM

MR SOUTHERN TO MR WATT.*

DEAR SIR,

THE experiments, of which the particular circumstances are hereafter related, were made in 1803, with the view of ascertaining, chiefly, the density of steam raised from water under different pressures above that of the atmosphere, an apparatus having then been made for a different purpose, which seemed pretty well adapted to this object, as it did equally so to that of ascertaining the latent heat of steam.

It may be premised, that the thermometers employed in all the experiments which will be now related, were made and graduated with the greatest care, the tubes having been accurately measured as to the proportional capacity of their different parts, the boiling point of each ascertained, according to the rules prescribed by a committee of the Royal Society, in 1777, (viz. the bulbs and tubes being in steam when the barometer stood at 29.8 inches, this degree of temperature being called 212°,) and in all cases the bulb and the tube, as high as the mercury ascended in it, were kept in the steam or the water whose temperature was to be noted. This latter circumstance was effected in the case of steam, by sliding the tube of the thermometer through a stuffing-box, or collar, made tight, till the mercury in it could just be seen above it. The tube had known marks on it, from which measurements were taken to the mercury, and thence the temperature known.

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* In all the experiments of which an account is given in this letter, Mr Southern was assisted by Mr William Creighton.

The quantity of steam was measured by filling a cylinder with it, (inclosed in the steam,) whose diameter was about 3.16 inches, and driving it out by the motion of a piston, which had 18 inches stroke regulated by the rotation of a crank. The solid contents of the piston-rod, which was 0.86 inches diameter, diminished the contents of the cylinder, leaving the quantity discharged each stroke by the motion of the piston, very nearly 130.7 cubic inches; but as the piston did not rise high enough to touch the top that closed the cylinder, and there was also unavoidably a space between the valve and the cylinder, these spaces together were computed to equal 1.7 cubic inches. Of course, had the elasticity of the steam been just equal to that of the atmosphere, no addition to the 130.7 cubic inches would need to be made; but as in the three successive experiments it was about $\frac{1}{3}$, $\frac{5}{3}$, and $\frac{9}{3}$ greater, these proportions of the spaces would escape when the valve was open that allowed the discharge of the steam to be made into the atmosphere, and must therefore be added respectively to the contents discharged by the motion of the piston.

These additional quantities are $1.7 \times \frac{1}{3} = 57$; $1.7 \times \frac{5}{3} = 2.83$; and $1.7 \times \frac{9}{3} = 5.1$; which, added to 130.7, gives 131.27 in the first experiment; 133.53 in the second, and 135.8 in the third, for the quantity of steam discharged at each stroke of the piston; and therefore the number of strokes which would discharge one cubic foot in each of the three experiments, would be 13.164, 12.941, and 12.724, respectively.

The steam was conducted from the cylinder, after passing the valve, by means of an iron pipe attached to a small copper one, having its end bent down, and immersed a short depth into a cistern of water. The cistern was made of fir-wood, and painted inside and outside with white paint; was about 30 inches square, and 26 inches deep; and the quantity of water in it was ascertained by weighing it, as was also the accession to it by the condensed steam.

The elasticity of the steam was ascertained by measuring an actual column of mercury which it supported; and the number of strokes was ascertained by a machine called a counter.

The following table contains the principal facts of these experiments.

Number of the Experiment. I.	Duration of the Experiment in Minutes. II.	Whole Number of Strokes. III.	Number of Strokes per Minute. IV.	Weight of Water in Cistern at beginning. V.	Temperature of Ditto. VI.	Weight of Water gained by condensed Steam. VII.	Temperature of Water in Cistern at the end. VIII.	Temperature gained. IX.	Elasticity of Steam in Boiler. X.	Temperature of Ditto. XI.
				lb.	°	lb.	°	°	Inches merc.	°
1	121¾	5154	42.3	721¾	45⅔	20.25	76	30⅓	40	229
2	51½	2434	41¼	722	48	20.00	80¼	32¼	80	270
3	38¼	1599	41.8	722	48	19.45	79¾	31¾	120	295

If the whole number of strokes in each experiment be divided by the number, found as above, that were required to discharge one cubic foot of steam, the whole number of cubic feet of steam discharged in each experiment will be given; viz. 5154 ÷ 13.164 = 391.53; 2434 ÷ 12.941 = 188.09; and 1599 ÷ 12.724 = 125.66; the quantity of steam formed and discharged in the first, second, and third experiments respectively, in cubic feet.

If the weight of water gained by the condensation of steam in each experiment, be multiplied by 27.65, the number of cubic inches of water in a pound weight, and divided by the number of cubic feet of steam which were condensed, the quotient will give the portion of water, in cubic inches, required by each cubic foot of steam for its formation; and hence also the comparative density.

$$\text{Thus}\quad \left.\begin{array}{r} 20.25\times 27.65\div 391.53=1.430 \\ 20.00\times 27.65\div 188.09=2.940 \\ 19.45\times 27.65\div 125.66=4.279 \end{array}\right\} \begin{array}{l}\text{inches of water to} \\ \text{form each cubic} \\ \text{foot of steam;}\end{array}$$

and these numbers are proportional to $\left\{\begin{array}{c} 40.00 \\ 82.24 \\ 119.70 \end{array}\right\}$ the relative densities, while the elasticities were as $\left\{\begin{array}{c} 40 \\ 80 \\ 120 \end{array}\right\}$ respectively.

These results appear to support the conclusion that *the density of steam is nearly, if not accurately, proportional to its elasticity;* at least this may be affirmed of it within the limits of these experiments.

From the above experiments may be calculated the latent heat of steam developed in the three cases; for if the weight of the water which received the augment of heat, be multiplied by the number of degrees of temperature communicated to it, and if this product be divided by the accession of weight to the water, (which only could have communicated the accession of temperature) it is evident that the quotient will give the temperature which the steam lost; and if to this be added the temperature which it retained, (viz. that of the water in the cistern at the conclusion of the experiment) a number will be obtained shewing the *whole heat*, or the *sum* of the latent and sensible heat of the steam. Hence, by substracting the sensible heat of the steam from this sum, the latent heat will be found. That is, $\frac{\text{col. 5} \times \text{col. 9}}{\text{col. 7}} + \text{col. 8} =$ the sum of the latent and sensible heat; or,

if W = weight of cold water, T = its temperature
w = accession of water by the condensed steam,
t = the temperature of warm water, and
x = the sum of the latent and sensible heat of the steam condensed.

$$\frac{\overline{W+w}.\, t - W\,T}{w} = x.$$

Either of these equations will be found to give in the three experiments 1157°, 1244°, and 1256°, from which substract the numbers, col. XI. 229°, 270°, and 295°, and there remain - - - - - 928°, 974°, and 961°, the latent heat.

Three other experiments were instituted with the intention of ascertaining the latent heat of steam under the three same degrees of elasticity, viz. equal to the support of 40, 80, and 120 inches of mercury. The steam was raised or generated in the same boiler used in the previous experiments, and from the end of a cast-iron pipe of 1½ inch diameter which united with it, a small copper pipe was taken (its diameter about ⅝ inch) and bent down so that its end could conveniently be immersed an inch or two under the surface of water. The end of this pipe was closed by a thin disk of copper, in which a circular hole was made $\frac{68}{1000}$ of an inch diameter, through which the steam from the boiler was blown into the cold water. The water which received the heat was contained in a tinned iron vessel that weighed 3.77 lbs., and its capacity for heat may therefore be called equivalent to ½lb. of water. In each of the experiments, the water employed to receive the steam weighed

28lb., to which, in the following table, recording the principal facts, is added this ½lb., in lieu of the vessel.

Number of Experiment.	Duration of the Experiment.	Weight of Cold Water.	Temperature of Ditto.	Temperature at the end of Experiment.	Temperature gained.	Weight of Water gained.	Temperature of Steam.	Elasticity of Ditto.
	′ ″	lbs.	°	°	°	lbs.	°	Inches.
1	12.45	28½	48	80	32	,878	229	40
2	5.50	28½	48	81⅓	33⅓	,857	270	80
3	4.00	28½	47¾	81	33¼	,826	295	120

If either of the equations above be applied here to the facts noted in this table, the sum of the latent and sensible heat will come out 1119°, 1190° and 1228°; and the latent heat 890°, 920°, and 933°. It was observed, however, that the tin vessel lost heat to the surrounding air very sensibly, and an experiment was made to determine the amount of this effect; and it was found when the contained water was at 80°, 1° was lost in five minutes; when at 60°, 1° was lost in 10½ minutes; it would therefore probably lose 1° in 8 minutes during the time of an experiment, the mean temperature being about 65°; and as the excess of temperature at the beginning and end of an experiment above that of the air was nearly the same in all three, the loss would be nearly proportional to the duration of each. Hence, to the acquired heat should be added, in the first experiment, 1⅝°; in the second, ¾°; and, in the third, ½°; being severally proportional to the said duration. These being respectively added to the temperatures in columns V. and VI., give in the former 81⅝°, 82° and 81½°; and, in the latter, 33⅝°, 34° and 33¾°; and if either of these sets of numbers be used in the calculation, according as one or the other of the equations is adopted to develope the results, they will be found to be 1171°, 1212° and 1245° for the sums of the latent and sensible heat; and consequently the latent heat in each experiment will be 942°, 942° and 950°.

It may be remarked, that no allowance was made, in calculating from the former experiments, for the heat which would be taken by the cistern, but which in the first of them, lasting two hours, would probably be very sensible, and may account for the principal part of the deficiency of latent heat brought out by the calculation from that experiment, in comparison with that from the two succeeding ones.

The opinion which I entertain from these experiments as to the *latent heat* of steam is, that it is a *constant* quantity, and perhaps this opinion obtains support from the modern discoveries of definite proportions. But it is necessary, however, to explain the limitation with which I here use this term, "*constant quantity.*" It is well known that if common air be expanded, cold is produced;* and it must therefore happen, that if a given quantity of it at a given temperature, could be gradually expanded, and as it was so expanded, gradually supplied with heat, so as to keep the temperature unaltered, this supply of heat would become *latent*; the thermometer would not shew it. It is probable, both from analogy and experiment, that this effect takes place in the expansion of steam. It is *not of this part* of its heat, though latent, and, in the experiments above related, undistinguishable, that I would be understood to speak when I state my opinion to be as just mentioned; but it is of that which, when water alters its state to that of an elastic fluid, becomes essential to it in every degree of elasticity, besides that which belongs to its expanded state. This latter may be called the latent heat of *expansion*,[*] while the other may perhaps properly be called *constitutional*.

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* An opportunity occurred to me some years back, which enabled me to determine, with tolerable precision, the degree of cold produced by the expansion of common air from the bulk of two to three, which I found to be 19° or 20°.

Allow me here to illustrate hypothetically this matter: If this essential or constitutional part of *latent* heat be added to water having the necessary portion of *sensible* heat, and perfectly confined in a close vessel, I conceive the water would be in the state of an elastic fluid; would in fact be steam, as dense as water; (possibly compressible, and capable of greater density) and would then require no latent heat of expansion; but if the containing vessel be now conceived to expand, for instance into double the space, I then imagine it would require some addition of heat during this expansion to maintain its proportional elasticity. It must be observed, however, that while this expansion was calling for more latent heat, the sensible heat necessary for the diminishing elasticity would be lessening; but it does not follow that these quantities should necessarily balance each other.[†]

When this fluid, steam, is raised in low temperatures, and of course under a low degree of elasticity, it obtains from its source, at the same instant, not only the constitutional part of its latent heat, but also that of expansion, and thus the two kinds are confounded; and, in experiments where they are developed by total condensation, are only to be detected together in sum; and it *may be* that this sum, together with the sensible heat, in different states of elasticity, may make a constant quantity; but, if the latent heat of expansion from a denser to a rarer state, be greater than the diminution of the sensible heat necessary only for the latter, the sum of the sensible and total latent heat will be more in steam raised in low temperatures than in high ones, which the result of your experiments made in low temperatures seems to countenance.

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[*] I have no view here to any substances not having the natural power of expansion, as water, ice, &c. J. S.

[†] I have, for very many years, entertained a similar hypothesis; but I know of no experiment whereby the truth of it can be demonstrated conclusively." W.

In all that I have said above, when speaking of steam, I have always intended that fluid in the state in which it is raised from water, viz. saturated therewith; but undoubtedly this fluid, after it is so raised under any temperature, and being clear from any additional accession of water, may be heated above that temperature, and cooled down to it again with changes of elasticity corresponding to those of temperature; like as common air may be, without limitation of temperature, as far as is known. This, however, is a view of the subject which has been totally excluded.

Besides the experiments first related, in which the temperature of steam raised under *high* pressures was observed, others had been made some years before (in 1797 and 8) for that purpose only; and as they were made with the greatest circumspection, both the manner of making them and their results may be here described, as may also the results of other experiments, made with equal care, to ascertain the temperature of steam raised under *low* pressures.

The instrument used in the former was a Papin's digester, similar to what you had used in your original experiments, and to that described in the "Encyclopædia Britannica," art. *Steam*, No. 22, the leading differences being in adapting a metallic tube to it to contain the thermometer, or rather as much of it as contained mercury, in the manner mentioned in the beginning of this letter, and instead of a valve, by the load on which to measure the elasticity of the contained steam, a nicely bored cylinder was applied, with a piston fitting it, so as to have very little friction, and to the rod of this was applied a lever, constructed to work on edges like those of a scale-beam, by which the resistance against the elastic force of the steam could be accurately determined; and at your suggestion, to be assured that no inaccuracy had crept into the calculation, by which this resistance through the medium of the lever was ascertained, an actual column of mercury of 30 inches high was substituted, and the correspondence was found to be within $\frac{1}{100}$ of an inch.

The observations at each of the points of pressure noted were continued some minutes, the temperature at each being alternately raised and lowered, so as to make the pressure of the steam on the under side of the piston alternately too much and too little for the weight with which it was loaded; and thence a mean temperature was adopted, the extremes of which were, as well as I now recollect, not more than half a degree on each side of it. The load on the piston, including its own weight, &c. &c. was calculated to be successively just equal to 1, 2, 4, and 8 atmospheres of 29.8 inches of mercury each, and the temperature of the steam was varied as above till that of each point was determined; the results were thus,

Atmospheres.	Pressure in Inches of Mercury.	Temperatures.
1	29..8	212°
2	59..6	250.3
4	119..2	293.4
8	238..4	343.6

The experiments for ascertaining the temperature of steam below the atmospheric pressure were made with an apparatus essentially similar to that which you originally used, and with scrupulous care and attention: and I met with the same incidents as you had done: such as, the production of a bubble of air whenever, after any experiment, the tube was inclined to refill the ball; and also the extraordinary suspension of a column of mercury of 35 inches vertical height, and of 7 inches of water above that, although the counterpoise was only that of the atmosphere, then under 30 inches. I found also that the tube required a considerable degree of tabouring or shaking to make the column subside and leave a space in the ball. This phenomenon was not produced till after much pains taken in inverting and re-inverting the tube again and again, nor till it had been suffered, after these operations, to stand for three or four days undisturbed in the exhausting position, and then discharging the air that had been accumulating in the interval.

The results, to be found in the table below, were deduced from the observations as you had done, viz. by adding to the height of the column of mercury in the tube (ascertained by a gauge floating on the surface of the mercury in the basin,) that of the water above it, or rather of an equivalent column of mercury, and substracting their sum from the height of the common barometer at the time. All these results were taken from observations made after the apparatus had been so perfectly exhausted of air as to produce the phenomenon just mentioned.

Temp.	Elasticity.			
	1st Set.	2d Set.	3d Set.	Mean.
°	In.	In.	In.	In.
52	0.	0.42	0.40	0.41
62	0.53	0.52	0.52	0.52
72	0.73	0.73	0.73	0.73
82	1.03	1.02	1.02	1.02
92	1.42	1.41	1.42	1.42
102	1.98	1.92	1.95	1.95
112	2.67	2.63	2.66	2.65

Temp.	Elasticity.			
	1st Set.	2d Set.	3d Set.	Mean.
°	In.	In.	In.	In.
122	3.58	3.54	3.58	3.57
132	4.68	4.65	4.72	4.68
142	6.05	6.00	6.14	6.06
152	7.86	7.80	7.89	7.85
162	9.98	9.96	10.04	9.99
172	12.54	12.72	12.67	12.64
182	16.01	15.84	15.88	15.91

The following formula will be found to give the elasticity belonging to a given temperature, and *vice versa*, with a sufficient degree of accuracy for most purposes, within the range of the experiments, at least, from which they have been formed.

Let t = temperature, e = elasticity, in inches of mercury;

$T = t + 52$, and $E = e - \frac{1}{10}$, $m = 94250{,}000000$:

$$\text{Then} \quad \frac{T^{5.14}}{m} = E$$

$$\sqrt[5.14]{Em} = T.$$

But as the calculation is most easily performed by logarithms, let L signify the logarithm of the quantity to which it is prefixed:

$$\text{Then} \quad 5.14\,LT - 10.97427 = LE$$

$$\frac{LE + 10.97427}{5.14} = LT.$$

The following table shows the observed elasticities, those derived from calculation by the formula, and the differences of the two, which appear to me to be as small as can be expected, taking a general view.

Temperature.	Observed Elasticites.	Calculated Elasticities.	Differences.
°	In.	In.	In.
32		0.18	
42		0.25	
52		0.35	
62	0.52	0.50	—0.02
72	0.73	0.71	—0.02
82	1.02	1.01	—0.01
92	1.42	1.42	0.00
102	1.95	1.96	+0.01
112	2.65	2.67	+0.02
122	3.57	3.58	+0.01
132	4.68	4.74	+0.06
°	In.	In.	In.
142	6.06	6.20	+0.14
152	7.85	7.99	+0.14
162	9.99	10.19	+0.20
172	12.64	12.86	+0.22
182	15.91	16.08	+0.17
192	. . .	19.91	. . .
202	. . .	24.45	. . .
212	29.80	29.80	0.00
250.3	59.60	59.69	+0.09
293.4	119.20	118.32	—0.88
348.6	238.40	237.60	—0.80

I believe it is now generally considered that the temperature 212° is that of water boiling when the barometer is at 30 inches instead of 29.8; and if in the above algebraic expressions the following alterations be made, the results from the formulæ will correspond with the adjustment of that point, and fully as well with the experiments generally.

Let $T = t + 51.3$; the index of the power and of the root be 5.13, instead of 5.14; and $m = 87344{,}000000$. So the two last equations will be: $5.13\ LT - 10.94123 = LE$; and $\frac{LE + 10.94123}{5.13} = LT$.

The table will stand as follows, supposing the thermometer had been graduated for 212° to correspond with 30 inches of the barometer:

Temperature.	Observed Elasticities.	Calculated Elasticities.	Differences.
°	In.	In.	In.
32	*0.16	0.18	+0.02
42	*0.23	0.25	+0.02
52	*0.35	0.35	0.00
62	0.52	0.50	—0.02
72	0.73	0.71	—0.01
82	1.02	1.01	—0.01
92	1.42	1.42	0.00
102	1.96	1.97	+0.01
112	2.66	2.68	+0.02
122	3.58	3.60	+0.02
132	4.71	4.76	+0.05
142	6.10	6.22	+0.12
152	7.90	8.03	+0.13
162	10.05	10.25	+0.20
172	12.72	12.94	+0.22
182	16.01	16.17	+0.16
192	. . .	20.04	. . .
202	. . .	24.61	. . .
212	30.00	30.00	0.00
350.3	60,00	60.11	+0.11
293.4	120.00	119.17	—0.83
343.6	240.00	239.28	—0.72

I remain, with the greatest esteem and respect,

Dear Sir,

Your very obedient Servant,

Oakhill, 26th March, 1814. JOHN SOUTHERN.

To JAMES WATT, Esq. Heathfield.

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* These are inserted from numerous experiments made by Mr W. Creighton.

P. S. Some circumstances which occurred in the performance of the experiments (made in 1797 and 1798) of which the results are last related, suggested the trials of a mixture of air with the steam; and I made a few, not indeed with the greatest nicety, but as they furnished a strong probability that the following law of elasticity of a given mixture was either nearly or accurately correct, it may be of use to say, that the apparatus used in the steam experiments being prepared as if for a repetition of them, and as perfectly exhausted of air as for them, a known measure of common air was sent up the tube through the mercury and water, and took its place in the ball; the water surrounding which was heated, and its temperature observed at different periods as before; and indeed the process was precisely the same as the former, with the additional notice of the space in the ball occupied by the expanded air and steam jointly. This process was repeated three or four times with different quantities of air, but the notes not being preserved, I can only now mention the conclusion they induced me to form as to the law above mentioned, viz. that whatever the elastic force of the air admitted would be in its expanded state, supposing it dry and to occupy the whole empty space in the ball (not occupied by water), after adding for the increase of its elasticity by the increase of temperature, it was yet to be augmented by the elasticity which steam alone of the same temperature would possess, to give the elasticity of the mixture.

Let $b =$ bulk of air introduced into the ball, measured before its introduction.

$A =$ its elasticity (expressed in inches of mercury, or the height of the common barometer).

$B =$ bulk occupied by it jointly with the steam in the ball, when their common temperature, governed by that of the water in the pan, is t.

$1 : r$ = ratio of elasticity which the air had before introduction to what it would have by augmenting its temperature to t; or, which is nearly the same, the ratio of expansion of air by the augment of temperature to t, when under the same pressure.

E = pillar of mercury (in inches) which steam of the temperature t would support:

Then $E+\frac{2bA}{B}$ = pillar of mercury (in inches) which would be supported by the elasticity of the mixture at the temperature t.

Example. Suppose $b=\frac{1}{2}$ cubic inch; $A=30$ inches; $t=102°$ 10 to 11, ratio of elasticity of air at the temperature at which it was introduced to what it would possess at 102, consequently $2=1.1$; $B=6$ inches, and E (by the table) 1.95. Then $1.95+\frac{1}{2}\times 30\times 1.1\div 6=4.70$ for elasticity of the mixture, or column of mercury it would support.

REFERENCE TO THE PLATES.

PLATE I.

Fig. 1. Pulse-glass, page 14.

2. 3. 4. Apparatus for experiments on the elasticity of steam, p. 23, 27, and 38.

5. Theory of the Geyser fountain, p. 43.

6. SAVARY's Steam-Engine, p. 50.
A, The Boiler.
V, Safety-valve.
B, Steam-pipe.
C, Steam-cock.
R, Receiver.
D, Injection-cock.
HG, Suction-pipe; G, its valve.
IK, Ascending-pipe. I, Valve.

7. NEWCOMEN's Steam-Engine, p. 59.
A, Boiler.
N, Steam-regulator.
Q, Steam-pipe.
CB, Cylinder.
P, Piston. PD, Piston-rod.
S, Injection-cock.
W, Injection-cistern; *f*, Snifting-valve.
d e g, Eduction-pipe. *h*, Valve.
HK, Working-beam; O its gudgeon.
FD EG, Arched-heads.
i k, Injection-pump.
M, Great-pump; XL, its rod.

Reference to Plate I.

Fig. 9. Mr WATT's Steam-Engine.
A, Boiler.
BC, Steam-pipe.
I, Steam-valve. L, Exhaustion-valve.
G, Cylinder; *g h*, its cover.
P, Piston.
D, Piston-rod.
OQR, Eduction-pipe.
a b o, Injection-pipe.
ST, Air-pump.
XY, Hot-water pump.

Reference to Plate II.

Fig. 8. BEIGHTON's Steam-Engine.
A′, Boiler.
Z, Steam-regulator.
K, Steam-pipe.
B′ B′, Cylinder.
P, Piston.
X, Piston-rod.
R, Injection-cock. QM, Injection-pipe.
C C, Injection-cistern.
W, Snifting-valve.
s, Eduction-pipe.
i k, Injection-pump.
M, Great-pump.
K K, Working-beam.

Fig. 10. On the expansion of steam in a cylinder.

Reference to Plate III. MR WATT's *Double Steam-Engine, from his Specification of* 1782.

Fig. 11. A section; Fig. 12. A front view of the cylinder, pipes, air-pump, &c. A, The piston.

B, Cylinder. 1, 1, Steam-case surrounding it.
C, 8, 9, Steam-pipe from the boiler.
D. Place of the upper steam-regulator, or valve.
N, Upper exhausting, or condensing-valve.
E, Lower steam-valve.
F, Lower exhausting-valve.
G, Eduction-pipe and condenser.
H, Injection-pipe and jet of water.
L, Valve between the condenser and air-pump.
K, Air-pump, with its bucket.
M, Box containing a valve similar to L.
P, Piston-rod.
O, A toothed rack; Q, A sector fixed on the main lever, or beam.
Y, A pinion.
X, A reciprocating fly-wheel.
R, Arch-head.
S, Pump-rod.
T, Connecting-rod.
W, A toothed-wheel.
U, Another toothed-wheel.
V, Rotative fly-wheel.
2, 3, Platform and beams for fixing the cylinder.
4, Wall of the condensing-cistern.
5, 6, Walls of the house.
7, Windows and doors.

Fig. 13. Section of a regulator-box, H, H, J, J.
H H, Opening from the cylinder; K, Cover of the box.
A, Spindle, passing through one side of the box.
B, Toothed sector, which works into the toothed rack C.
D, The valve; E, Its seat; F, Guide for the stem of the valve.
G, Pipe leading to the condenser.

Fig. 14. Manner of the action of the plug-tree on the levers of the regulating-valves.
Q R, Part of the plug-tree; P, One of its pins.
N O, Lever, moveable on the axis N.
N M, An arm attached to the same axis, with a joint at M, connecting it with the rod M L.
L A, Lever, fixed on the spindle A.

Reference to Plate IV. of MR WATT'S *Single Reciprocating Engine.*

A, The Cylinder.
B, Piston.
C, Piston-rod.
D, Stuffing-box on the cylinder cover.
E, Steam-case of the cylinder.
F, Steam-pipe from the boiler.
G, Steam-nozle and valve, or regulator.
I, Perpendicular steam-pipe.
J, Eduction-pipe.
K, Equilibrium-valve and nozle.
L, Exhaustion-valve and nozle.
M, Condenser.
N, Injection-cock.
O, Blow-valve pipe.
P, Air-pump.
Q, Air-pump bucket and rod.
R, Lower valve of the air-pump.
S, Upper valve. (The hot-water pump, represented in section of Albion-Mill engine, was discontinued about this time.)
U, Pump for supplying boiler.
V, Injection, or cold-water pump.
Y, Working-gear of the nozle, or regulator valves.
Z, Plug-tree, which acts on the working-gear.
a, Main lever, or working-beam.
b, Gudgeon of the main-lever.
c, Straps for strengthening the beam. d, King-post.
e, Arch-heads.
f, Chains for the piston-rod and pump-rod.
g, Arch-heads and chains for the air-pump.
h, Rod of the great pump.
i, Bucket.
j, Working barrel of the pump.
k, Clack.
n, Boiler. o, Flues on the sides of it.
p, Grate.
r, Damper. s, Chimney.
t, Feed-pipe.

Reference to Plates V. VI. VII. VIII. of one of the Albion-Mill Steam-Engines.

(All the Letters are not to be found on any one of the Plates.)

A, The cylinder, 34 inches diameter.
B, Piston, which makes a stroke of eight feet.
C, Piston-rod.
D, Stuffing-box on the cylinder cover.
E, Steam-case of the cylinder; *e*, syphon to empty it of water.
F, Steam-pipe from the boiler; *f*, a pipe to supply cylinder-case.
G, Upper steam-nozle and valve, or regulator-box and regulator.
H, Upper exhaustion-nozle and valve.
I, Perpendicular steam-pipe.
J, Eduction-pipe.
K, Lower steam-nozle and valve.
L, Lower exhaustion-nozle and valve.
M, Condenser, immersed in a cistern of cold water.
N, Injection-cock (always open when the engine works.)
O, Blow-valve.
P, Air-pump.
Q, Lower valve of air-pump.
R, Air-pump bucket and rod.
S, Upper valve of air-pump.
T, Hot-water pump, with its bucket and rod.
U, Cold-water pump.
V, Pump for supplying boiler.
W, Governor, turned by a band from the fly-wheel shaft.
X, Lever and rod to connect governor with the throttle-valve at *t*.
Y, Working-gear of the nozle-valves.
Z, Plug-tree, which acts on the working-gear.
a, Main lever, or working-beam.
b, Its main gudgeon.
c, Perpendicular links of the parallel motion.
d, Parallel bars.
e, Regulating radiuses.
f, Small perpendicular links.
g, Secondary parallel motion for the air-pump.
h, Connecting-rod.
i, Planet-wheel, fixed to the connecting rod.
j, Sun-wheel.

k, Fly-wheel shaft, on which the sun-wheel is fixed.
l, Connecting-link, to retain the planet-wheel in its orbit.
m, Fly-wheel.
n, Boiler.
o, Tube through the boiler and flues round it.
p, Grate.
q, Feeding-mouth.
r, Damper.
s, Chimney.
t, Feed-pipe.
u, Gage-pipes and cocks.
v, Safety-valve.
w, Bevil-wheels of the mill.
x, Upright shaft.
y, Large spur-wheel.
z, Pinions.
* Millstones.

Description of the Method of Working the Albion-Mill Engines. Plates V. VI. VII. VIII.

The steam-pipe F (Plate VI.) conveys steam from the boiler *n* to the cross-pipe, or upper steam-nozle G, and by the perpendicular steam-pipe I, to the lower steam-nozle K. In the nozle G is a valve, which, when open, admits steam into the cylinder *above* the piston B, (Plate V.) through the horizontal square pipe at its top; and in the *lower* steam-nozle K there is another valve, which, when open, admits steam into the cylinder *below* the piston. In the upper exhaustion-nozle H is a valve, which, when open, admits steam to pass from the cylinder *above* the piston into the exhaustion-pipe J, which conveys it to the condensing-vessel M, where it meets the jet of the injection from the cock N, and is reduced to water; and, in the *lower* exhaustion-nozle L, there is also a valve, which, when open, admits steam to pass out of the cylinder *below* the piston, by the eduction-pipe, into the condenser M.

The engine being at rest, the cylinder quite cold, and the condenser-cistern full of water, when the water in the boiler begins to boil, steam will enter by the small pipe *f* (Plate VI.) into the space between the cylinder and the heating-case E, which will expel the air contained in that space, and between the two bottoms of the cylinder, at a cock fixed in the outer bottom, which, when all the air is expelled, and the cylinder thoroughly warmed, is to be shut, and the water which may be formed in these spaces during the working of the engine, will issue by the inverted syphon *e*.

Things being in this situation to produce a commencement of motion, the first operation is to open all the four valves, G, H, K, L; (the injection-cock being shut) the steam will drive the air out of the steam and exhaustion-pipes I and *J*, and out of the condenser M, through the blow-pipe and its valve O, (Plate VI.) and as soon as this is succeeded by a sharp crackling noise in the little cistern O, the valves are to be shut until it is thought that the steam which has entered is mostly condensed.

The same operation is to be repeated, giving a longer time to cool between the times of blowing, until it is found that, upon opening the injection-cock, some water will enter, and the barometer shall shew some degree of exhaustion, after which, the repetition of blowing will soon empty the cylinder of air.

The piston being then at the top of its stroke, the valves G and L are to be opened, and the fly-wheel *m* (Plate V.) turned by hand about one-eighth of a revolution, or more, in the direction in which it is intended to move; the steam which is then in the cylinder will pass by L into the condenser, when, meeting the jet of water from the injection-cock, it will be converted into water, and the cylinder thus becoming exhausted, the steam, entering the cylinder by the valve G, will press upon the piston and cause it to descend, while, by its action upon the working-beam through the piston-rod, &c., it pulls down the cylinder-end of the beam, and raises up the outer-end and the connecting rod *h*, which causes the planet-wheel *i* to tend to revolve round the sun-wheel *j*; but the former of these wheels, being fixed upon the connecting-rod so that it cannot turn upon its own axis, and its teeth being engaged in those of the sun-wheel, the latter, and the fly-wheel, upon whose axle or shaft it is fixed, are made to revolve in the desired direction, and give motion to the mill-work.

As the piston descends, the plug-tree Z also descends, and a clamp, or slider *q*, (Plate V.) fixed upon the side of the plug-tree, presses upon the handle 1 of the upper Y-shaft, or axis, and thereby shuts the valves G and L, and the same operation, by disengaging a detent, permits a weight suspended to the arm of the lower Y-shaft to turn the shaft upon its axis, and thereby to open the valves K and H. The moment previous to the opening these valves, the piston had reached the lowest part of its stroke, and the cylinder *above* the piston was filled with steam; but as soon as H is opened, that steam rushes by the eduction-pipe J, into the condenser, and the cylinder *above* the piston becomes exhausted. The steam from the boiler entering by I and K, (Plate VI.)

acts upon the *lower* side of the piston, and forces it to return to the top of the cylinder. When the piston is very near the upper termination of its stroke, another slider *a* raises the handle 2, and, in so doing, disengages the catch which permits the upper Y-shaft to revolve upon its own axis, and open the valves G and L, and the downward stroke recommences as has been related.

When the piston descends, the buckets R, T of the air-pump P and hot-water pump T also descend. The water which is contained in these pumps passes through the valves of their buckets, and is drawn up and discharged by them through the lander or trough *t*, by the next descending-stroke of the piston. Part of this water is raised up by the pump V, for the supply of the boiler, and the rest runs to waste.

Particular Description of the Mechanism which opens and shuts the Nozle-valves or Regulators. Plate VII. Fig. 2.[*]

The piston approaching the top of the cylinder, the slider *a* fastened upon the plug-tree Z, raises up the handle *b*, which is fixed upon the lower Y-shaft, or axis *c*, as is the detent *d*, and the latter takes hold of the double-ended catch *e*; but, in doing this, the upper end of the catch allows the detent *f* to escape, and a weight hung to the rod *g* turns the axis *h*. The arm *i*, and rod *j*, are moved out of the straight line at *l*, and by a lever *k* turn the spindle *m* in the upper nozle, which, by means of a toothed-sector *n*, and rack *o*, raises the valve *p*, and admits steam into the cylinder *above* the piston through the horizontal pipe A. At the same time, another arm *u*, fixed upon the same shaft, by means of the rod *w*, acts upon a spindle, &c., in the lower nozle, and opens the exhaustion-valve L, (Plate V.) and thereby forms a communication between the cylinder *below* the piston and the condenser. The piston now descending, another slider *q*, moves the handle *r* into the position *s*; this raises the weight *g*, while *i* and *k* are brought back to the position *l*, and the valves *p* and L are shut. The detent *f*, in acting upon the catch *e*, disengages *d*; the lower Y-shaft turns upon its axis, and two arms attached to it (similar to those upon the upper Y-shaft, which are omitted to avoid confusion) by means of the rod *x* and *y*, open the lower steam-valve K, (Plate V.) and the upper exhaustion-valve *t*.

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[*] In the plan, only one of the Y-shafts is shown, and the levers which open and shut the steam-valve of the upper nozle.

The cylinder above the piston becomes exhausted, and the steam, entering below it, causes the piston to reascend.*

Description of the Governor.

The governor W (Plate V.) consists of a perpendicular axis which is put in motion by means of a band passing round a pulley fixed upon it, and round another pulley fixed upon the main-shaft *k*, (Plates VII. and VIII.) Two heavy balls are appended to this axis by means of two rods which cross each other in a mortise formed in the upper part of the axis. After crossing each other, these two rods are bent outwards in a contrary direction, and to them are jointed two short pieces, the other ends of which join to a collar capable of sliding up and down upon the upper part of the axis, forming altogether an assemblage resembling one of the joints of a tobacco tongs; so that, when the balls recede, which happens from the centrifugal force given by any increase of velocity of the main-shaft, the collar is depressed, and, *vice versa*, raised when the velocity diminishes; and, by its action upon the double-ended lever *x*, which is connected with an arm fixed upon the spindle *t* of the throttle-valve, admits more or less steam to the cylinder as the case requires.

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* This construction of the *nozle-regulators* and *working gear* was in use at the time Dr Robison composed his Essay, but several material alterations have since been made in it; an account of which I must leave to some future historian, as they do not fall within the scope of these annotations upon Dr R.'s Essay.

MACHINERY.

The term *Machine* is now vulgarly given to a great variety of subjects, which have very little analogy by which they can be classed with propriety under any one name. We say a travelling machine, a bathing machine, a copying machine, a threshing machine, an electrical machine, &c. &c. The only circumstance in which all these agree seems to be, that their construction is more complex and artificial than the utensils, tools, or instruments, which offer themselves to the first thoughts of uncultivated people. They are more artificial than the common cart, the bathing tub, or the flail. In the language of ancient Athens and Rome, the term was applied to every tool by which hard labour of any kind was performed; but, in the language of modern Europe, it seems restricted either to such tools or instruments as are employed for executing some philosophical purpose, or of which the construction employs the simple mechanical powers in a conspicuous manner, in which their operation and energy engage the attention. An electrical machine, a centrifugal machine, are of the first class; a threshing machine, a fire machine, are of the other class. It is nearly synonymous, in our language, with ENGINE; a term altogether modern, and in some measure honourable, being bestowed only, or chiefly, on contrivances for executing work in which ingenuity and mechanical skill are manifest. Perhaps, indeed, the term *engine* is limited, by careful writers, to machines of considerable magnitude, or at least of considerable art and contrivance. We say, with propriety, steam-engine, fire-engine, plating-engine, boring-engine; and a dividing machine, a copying machine, &c. Either of these terms, *machine* or *engine*, are applied with impropriety to contrivances in which some piece of work is not executed on materials which are then said to be manufactured. A travelling or bathing machine is surely a vulgarism. A machine or engine is therefore a TOOL; but of complicated construction, peculiarly fitted for expediting labour, or for performing it, according to certain invariable principles: and we should add, that the dependence of its efficacy on mechanical principles must be apparent, and even conspicuous. The contrivance and erection of such works constitute the profession of the engineer; a profession which ought by no means to be confounded with that of the mechanic, the artisan, or manufacturer.

By far the greatest number of our most serviceable engines consist chiefly of parts which have a motion of rotation round fixed axes, and derive all their energy from levers virtually contained in them. And these acting parts are also material, requiring force to move them over and above what is necessary for producing the acting force at the working part of the machine. The modifications which this circumstance frequently makes of the whole motions of the machine, are indicated in the article ROTATION (see vol. i.) in an elementary way; and the propositions there investigated will be found almost continually involved in the complete theory of the operation of a machine. Lastly, it will be proper to consider attentively the propositions contained in the article STRENGTH *of Materials*, that we may combine them with those which relate wholly to the working of the machine; because it is from this combination only that we discover the strains which are excited at the various points of support, and of communication, and in every member of the machine. We suppose all these things already understood.

78. Our object at present is to point out the principles which enable us to ascertain what will be the precise motion of a machine of given construction, when actuated by a natural power of known intensity, applied to a given point of the machine, while it is employed to overcome a known resistance acting at another point. To abbreviate language, we shall call that the IMPELLED POINT of the machine to which the pressure of the moving power is immediately applied; and we may call that the WORKING POINT, where the resistance arising from the work to be performed immediately acts.

To consider this important subject, even in its chief varieties, requires much more room than can be allowed in a work like this, and therefore we must content ourselves with a very limited view; but, at the same time, such a view as shall give sufficient indication of the principles which should direct the practical reader in every important case. We shall consider those machines which perform their motions round fixed axes; these being by far the most numerous and important, because they involve in their construction and operations all the leading principles.

79. That we may proceed securely, it is necessary to have a precise and adequate notion of moving force, as applied to machinery, and of its measures. We think this peculiarly necessary. Different notions have been entertained on this subject by Mr Leibnitz, Des Cartes, and other eminent mechanicians of the last century; and their successors have not yet come to an agreement. Nay, some of the most eminent practitioners of the present times, (for we must include Mr Smeaton in the number,) have given

measures of mechanical power in machinery which we think inaccurate, and tending to erroneous conclusions and maxims.*

We take for the measure (as it is the effect) of exerted mechanical power the quantity of motion which it produces by its uniform exertion during some given time. We say *uniform exertion*, not because this uniformity is necessary, but only because, if any variation of the exertion has taken place, it must be known, in order to judge of the power. This would needlessly complicate the calculations; but in whatever way the exertion may have varied, the whole accumulated exertion is still accurately measured by the quantity of motion existing at the end of the exertion. The reader must perceive that this is the same thing that is expressed in the article DYNAMICS, (vol. i. p. 108) by the area of the figure whose abscissa or axis represents the time of exertion; and the ordinates are as the pressures in the different instants of that time, the whole being multiplied by the number of particles, (that is, by the quantity of matter,) because that figure represents the quantity of motion generated in one particle of matter only. All this is abundantly clear to persons conversant in these disquisitions; but we wish to carry along with us the distinct conceptions of that useful class of readers whose profession engages them in the construction and employment of machines, and to whom such discussions are not so familiar. We must endeavour, therefore, to justify our choice of this measure by appealing to familiar facts.

If a man, by pressing uniformly on a mass of matter for five seconds, generates in it the velocity of eight feet per second, we obtain an exact notion of the proportion of this exertion to the mechanical exertion of gravity, when we say that the man's exerted force has been precisely one-twentieth part of the action of gravity on it; for we know that the weight of that body (or, more properly, its heaviness) would, in five seconds, have given it the velocity of 160 feet per second, by acting on it during its fall. But let us attend more closely to what we mean by saying that the exerted force is one-twentieth of the exertion of gravity. The only notion we have of the exertion of gravity is what we call the weight of the body—the pressure which we feel it make on our hand. To say that this is 20 pounds weight does not explain it; because this is only the action of gravity on another piece of matter. Both pressures are the same. But if the body weighs 20 pounds, it will draw out the rod of a steelyard to the mark 20. The rod is so divided, that the 20th part of this pressure will draw it out to 1. Now the fact is, that if the man presses on the mass of 20 pounds weight with a spring steelyard during five seconds, and if during that time the rod of the steelyard was always at the mark 1, the body will have acquired the velocity of eight feet per second. This is an acknowledged fact. Therefore we were right in saying, that the man's exertion is one-twentieth of the exertion of gravity. And since we believe the weight of bodies to be proportional to their quantity of matter, all matter being equally heavy, we may say, that the man's exertion was equal to the action of gravity on a quantity of matter whose weight is one pound. We express it much more familiarly, by saying, that the man exerted on it the pressure of one pound of matter, or the force of one pound.

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* A very able paper in support of Smeaton's measure of moving force, by Mr Ewart, will be found in the Manchester Memoirs, vol. ii. p. 113.—ED.

In this manner, the motion communicated to a mass of matter, by acting on it during some time, informs us with accuracy of the real mechanical force or pressure which has been exerted. This is judged to be double when twice the velocity has been generated in the same mass, or where the same velocity has been generated in twice the mass; because we know that a double pressure would have done either the one or the other.

But farther: we know that this pressure is the exertion;

we have no other notion of our own force; and our notion of gravity, of elasticity, or any other natural force, is the same. We also know that the continuance of this exertion fatigues and exhausts our strength, as completely as the most violent motion. A dead pull, as it is called, of a horse, at a post fixed in the ground, is a usual trial of his strength. No man can hold out his arm horizontally for much more than a quarter of an hour; and the exertion of the last minutes gives the most distressing fatigue, and disables the shoulder from action for a considerable time after. This is therefore an expenditure of mechanical power, in the strict primitive sense of the word. Of this expenditure we have an exact and adequate effect and measure in the quantity of motion produced; that is, in the product of the quantity of matter by the velocity generated in it by this exertion. And it must be particularly noticed, that this measure is applicable even to cases where no motion is produced by the exertion; that is, if we know that the exertion which is just unable to start a block of stone lying on a smooth stone pavement, but would start it, if increased by the smallest addition; and if we know that this would generate in a second 32 feet of velocity in a 100 pounds of matter, we are certain that it was a pressure equal to the weight of this 100 pounds. It is a good measure, though not immediate, and may be used without danger or mistake when we have no other.

80. The celebrated engineer, Mr Smeaton, in his excellent dissertation on the power of water and wind to drive machinery, and also in two other dissertations, all published in the Philosophical Transactions, and afterwards in a little volume, has employed another measure, both of the expenditure of mechanical power, and of the mechanical effect produced. He says, that the weight of a body, multiplied by the height through which it descends, while driving a machine, is the only proper measure of the power expended; and that the weight, multiplied by the height through which it is uniformly raised, is the only proper

measure of the effect produced. And he produces a large train of accurate experiments to prove that a certain weight, descending through a certain space, always produces the same effect, whether it has descended swiftly or slowly, employing little or much time.

Had this eminent engineer proposed this as a popular measure, of easy comprehension and remembrance, and as well accommodated to the uses of those engaged in the construction of machines, when restricted to a certain class of cases, it might have answered very good purposes; but the author is at pains to recommend it to the philosophers as a necessary correction of their theories, which, he says, tend to mislead the artists. His own reasonings terminate in the same conclusion with Mr Leibnitz's, namely, that the power of producing a mechanical effect, and the effect produced, are proportional to the square of the velocity. The deference justly due to Mr Smeaton's authority, and the influence of his name among those who are likely to make the most use of his instructions, render it necessary for us to examine this matter with some attention.

Mr Smeaton was led to the adoption of this measure by his professional habits. Raising a weight to a height is, in one shape or another, the general task of the machines he was employed to erect; and we may add, the opportunities of expending the mechanical powers of nature which are in our command, are generally in this proportion. A certain daily supply of water, coming from a certain height, is our best opportunity, and may very properly be said to be expended.

81. This being the general case, the measure was obvious, and natural, and good. The power and effect were of the same kind, and *must* be measures of each other; at least, in those circumstances in which they were set in opposition. Yet even here Mr Smeaton was obliged to make a restriction of his measures: "The height through which a body *slowly and equally* descended, or to which it was raised." And why was this limitation necessary? "Because in rapid

or accelerated motions, the inertia of bodies occasioned some variation." But this is too vague language for philosophical disquisition. Besides, what is meant by this variation? What is the standard from which the unrestricted measure varies? This standard, whatever it is, is the true measure, and it was needless to adopt any other. Now, the standard from which Mr Smeaton estimates the deviation, is the very measure which we wish to employ, namely, the quantity of motion produced. Strictly speaking, even this is not the immediate measure. The immediate measure is that faculty which we call pressure. This is the intermedium preceivable in all productions of motion; and it is also the intermedium of mechanical effect, even when motion is not produced; as when the weight of a body bends a spring, or the elasticity of a body supports another pressure. How it operates in all or any of these cases, we know not; but we know that all these measures of pressure agree with each other. A double quantity of motion will bend a spring doubly strong, will raise a double weight, will withstand any double pressure, &c. &c. In short, pressure is the immediate agent in every mechanical phenomenon. It penetrates bodies, overcoming their tenacity; it overcomes friction; it balances pressure; it produces motion. Mr Smeaton's measure is only nearly true, in any case; and in all cases it is far from being exact in the first instants of the motion, during its acceleration or retardation.

We have already noticed the complete expenditure of animal power by continued pressure, even when motion is not produced; the only difficulty is to connect this in a measurable way with the power which the same exertion has of generating motion in a body.

When a man supports a weight for a single instant, he certainly balances the pressure or action of gravity on that body; and he continues this action as long as he continues to support it: and we know that if this body were at the end of a horizontal arm turning round a vertical axis, the same effort which the man exerted in merely carrying the weight, if now exerted on the body, by pushing it horizontally round the axis, will generate in it the same velocity which gravity would generate by its falling freely. On this authority, therefore, we say, that the whole accumulated action of a man, when he has just carried a body whose weight is 30 pounds for one minute, is equal to the whole exertion of gravity on it during that minute; and, if employed, not to counteract gravity, but to generate motion, would generate, during that minute, the same motion that gravity would, that is, 60 × 32 feet velocity per second, in a mass of 30 pounds. There would be 30 pounds of matter moving with the velocity of 1920 feet per second. We would express this production or effect by 30 × 1920, or by 57600, as the measure of the man's exertion during the minute.

But, according to Mr Smeaton, there is no expenditure of power, nor any production of mechanical effect, in thus carrying 30 pounds for a minute; there is no product of a weight by a height through which it is equably raised; yet such exertion will completely exhaust a man's strength if the body be heavy enough. Here then is a case to which Mr Smeaton's measure does not readily apply; and this case is important, including all the actions of animals at a dead pull.

But let us consider more narrowly what a man really does when he performs what Mr Smeaton allows to be the production of a measurable mechanical effect. Suppose this weight of 30 pounds hanging by a cord which passes over a pulley, and that a man, taking this cord over his shoulder, turns his back to the pulley, and walks away from it. We know, that a man of ordinary force will walk along, raising this weight, at the rate of about 60 yards in a minute, or a yard every second, and that he can continue to do this for eight or ten hours from day to day; and that this is all that he can do without fatigue. Here are 30 pounds raised uniformly 180 feet in a minute; and Mr Smeaton would express this by 30 × 180, or 5400, and would call this the measure of the mechanical effect, and also of the expenditure of power. This is very different from our measure 57600.

82. But this is not an accurate and complete account of the man's *action* on the weight, and of the whole effect produced. To be convinced of this, suppose that a man A has been thus employed, while another B, walking alongside of him at the same rate, suddenly takes the rope out of his hand, frees him of the task, and *continues* to raise the weight without the smallest change on its velocity of ascent. What is the action of B, and whether is it the same with that of A or not? It is acknowledged by all, that the exertion of B against the load is precisely equal to 30 pounds. If he holds the rope by a spring steelyard, it will stand constantly at the mark 30. B exerts the same action on the load as when he simply supports it from falling back into the pit. It was moving with the velocity of three feet per second when he took hold of the rope, and it would continue to move with that velocity if any thing could annihilate or counteract its gravity. If, therefore, there was no action when a person merely carried it, there is none at present when it is rising 180 feet in a minute. The man does, indeed, work more than on that occasion, but not against the load: his additional work is walking, the motion of his own body, as a thing previously necessary that he may continue to support the load, that he may continue his mechanical effort as it follows him. It appears to yield to him; but it is not to *his* efforts that it yields; its weight completely balances those efforts, and is balanced by them. It was to a *greater* effort of the man A that it yielded. It was then lying on the ground. He pulled at the cord gradually, perhaps increasing his pull, till it was just equal to its weight. When this obtains, the load no longer presses on the ground, but is completely carried by the rope. But it does not move by this effort of 30 pounds; but let him exert a force of 31 pounds, and continue this for three seconds. He will put it in motion; will accelerate that motion; and, at

the end of three seconds, the load is rising with the velocity of three feet per second. The man feels that this is as much speed as he can continue in his walk; he therefore slackens his pull, reducing his action to 30 pounds, and with this action he walks on. All this would be distinctly perceived by means of a steelyard. The rod would be pulled out beyond 30, till the load acquired the uniform velocity intended, and after this it would be observed to shrink back to 30.

More is done, therefore, than appears by Mr Smeaton's measure. Indeed, all that appears in it is the exertion necessary for *continuing* a motion already produced, but which would be immediately extinguished by a contrary power, which must therefore be counteracted. This measure will not apply to numberless cases of the employment of machines where there is no such opposing power, and where, notwithstanding, mechanical power must be expended, even according to Mr Smeaton's measurement. Such are corn-mills, boring-mills, and many others.

How then comes it that Mr Smeaton's valuable experiments concur so exactly in shewing that the same quantity of water descending from the same height, always produces the same effect, (as he measured it) whatever be the velocity? In the first place, all his experiments are cases where the power expended, and the work performed, are of the same kind: A heavy body descends, and, by its preponderancy, raises another heavy body. But even this would not ensure the precise agreement observed in his experiments, if Mr Smeaton were not careful to exclude from his calculations all that motion where there is any acceleration, and all the expenditure of water during the acceleration, and to admit only those motions that are sensibly uniform. In moderate velocities, the additional pressure required for the first acceleration is but an insignificant part of the whole; and to take these accelerated motions into the account, would have embarrassed the calculations, and perhaps confused many of his readers. We see, in the instance now given, that the addition of one pound continued for three seconds only, was all that was necessary.

Mr Smeaton's measurement is therefore abundantly exact for practice; and being accommodated to the circumstances most likely to engage the attention, is very proper for the instruction of the numerous practitioners in all manufacturing countries who are employed for ordinary erections: But it is improperly proposed as an article essential to a just theory of mechanics, and therefore it was proper to notice it in this place. Besides, there frequently occur most important cases, in which the motion of a machine is, of necessity, desultory, alternately accelerated and retarded. We should not derive all the advantages in our power from the first mover, if we did not attend particularly, and chiefly, to the *accelerating* forces. And, in every case, the improvement, or the proper employment of the machine, is not attained, if we are not able to discriminate between the two parts of the mechanical exertion; one of them, by which the motion is produced and accelerated to a certain degree; and the other, by which that motion is continued. We must be able to appreciate what part of the effect belongs to each. But it is now time to proceed to the important question,

What will be the precise motion of a machine of a given construction, actuated by a power of known intensity and manner of acting, and opposed by a known resistance?

83. In the solution of this question, much depends on the nature of both power and resistance. In the statical consideration of machines, no attention is paid to any differences. The intensity of the pressures is all that it is necessary to regard, in order to state the proportion of pressure which will be exerted in the various parts of the machine. The pressures at the impelled and working-points, combined with the proportions of the machine, necessarily determine all the rest. Pressure being the sole cause of all me-

chanical action among bodies, any pressure may be substituted for another that is equal to it; and the pressure which is most familiar, or of easiest consideration, may be used as the representative of all others. This has occasioned the mechanical writers to make use of the pressure of gravity as the standard of comparison, and to represent all powers and resistances by weights. However proper this may be in their hands, it has hurt the progress of the science. It has rendered the usual elementary treatises of mechanics very imperfect, by limiting the experiments and illustrations to such as can be so represented with facility. This has limited them to the state of equilibrium, (in which condition a working machine is never found) because illustrations by experiment out of this state are neither obvious nor easy. It has also prevented the students of mechanics from accomplishing themselves with the mathematical knowledge required for a successful prosecution of the study. The most elementary geometry is sufficient for a thorough understanding of equilibrium, or the doctrines of statics; but true mechanics, the knowledge of machines as instruments by which work is performed, requires more refined mathematics, and is inaccessible without it.

Had not Newton, or others, improved mathematics by the invention of the infinitesimal analysis and calculus, we must have rested contented with the discoveries (really great) of Galileo and Huyghens. But Newton, *suâ mathesi facem præferente*, opened a boundless field of investigation, and has not only given a magnificent and brilliant specimen of the discoveries to be made in it, but has also traced out the particular paths in which we are to find the solution of all questions of practical mechanics. This he has done by shewing another species of equilibrium, indicated, not by the cessation of all motion, but by the uniformity of motion; by the cessation of all acceleration or retardation. As the extinction of motion by the action of opposite forces is assumed by us as the indication of the perfect equality of those forces, so the extinction of acceleration should be received as the indication of something equal and opposite to the force which was known to have caused the acceleration, and, therefore, as the indication of an equilibrium between opposite forces, or else of the cessation of all force.

84. This new view of things was the source of all our distinct notions of mechanical forces, and gave us our only unexceptionable marks and measures of them. The 39th proposition of the first book of Newton's Principles of Natural Philosophy, and its corollaries, contain almost the whole doctrine of active mechanical nature, and are peculiarly applicable to our present purpose, because they enable us to comprehend in this *mechanical equilibrium* (so different from the *statical*) every circumstance in which those pressures which are exerted by natural powers differ from each other, and vary in their action on the impelled and working-points of a machine. Indeed, when we recollect that the operations of our machines are the same on board a ship as on shore, and that all our machines are moving with the ground on which they stand, we must acknowledge, that even ordinary statics is only an imperfect view of an equilibrium among things which are in motion; and this should have taught us that, even in those cases where nothing like equilibrium appears, an equilibrium may still be usefully traced.

85. In the statical consideration of machines, the *quantity* of pressure is all that we need attend to. But, in the mechanical discussion of their *operations*, we must attend to their distinctions in kind: and it will by no means be sufficient to represent them all by weights; for their distinction in kind is accompanied by great differences in their manner of acting on the machine. Some natural powers, in order to continue their action on the impelled point of the machine, must, at the same time, put into motion a quantity of matter external to the machine in which these powers reside; and this must be made to follow the

impelled point in its motion, and not only follow, but continue to press it forward; or this matter, thus continually put into motion, must be successively applied to different points of the machine, which become impelled points in their turn. This is the case with a weight, with the action of a spring, the action of animals, the action of a stream of water or wind, and many other powers. A part of the natural mechanical powers must therefore be employed in producing this external motion. This is sometimes a very considerable part of the whole natural power. In some cases it is the whole of it. This obtains in the action of a descending weight, lying on the end of a lever and pressing it down, or hanging by a chord attached to the machine.

There is also an important distinction in the manner in which this external motion is kept up. In a weight employed as the moving power, the actuating pressure seems to reside in the matter itself; and all that is necessary for continuing this pressure is merely to continue the connection of it with the machine. But in the action of animals it may be very different: A man pushing at a capstan-bar, must first of all walk as fast as the bar moves round, and this requires the expenditure of his muscular force. But this alone will not render his action an effective power: He must also *press forward* the capstan-bar with as much force as he has remaining over and above what he expends in walking at that rate. The proportion of these two expenditures may be very different in different circumstances; and in the judicious selection of such circumstances as make the first of these as inconsiderable as possible, lies much of the skill and sagacity of the engineer. In the common operation of thrashing corn, much more than half of the man's power is expended in giving the necessary motion to his own body, and only the remainder is employed in urging forward the swiple with a momentum sufficient for shaking off the ripe grains from the stalk. We had sufficient proof of this by taking off the swiple of the flail, putting the same weight of lead on the end of the staff, and then causing the hind to perform the usual motions of thrashing with all the rapidity that he could continue during the ordinary hours of work. We never could find a man who could make three motions in the same time that he could make two in the usual manner, so as to continue this for half an hour. Hence we must conclude, that half (some will say two-thirds) of a thresher's power is expended in merely moving his own body. Such modes of animal action will therefore be avoided by a judicious engineer; but to be avoided, their inconvenience must be understood. More of this will occur hereafter. In other cases, we are almost (never wholly) free from this unprofitable expenditure of power. Thus, in the steam-engine, the operation requires that the external air follow the piston down the cylinder, in order to continue its pressure. But the force necessary for sending in this rare fluid into the cylinder with the necessary velocity, is such an insignificant part of the whole force which is at our command, that it would be ridiculous affectation in any engineer to take it into account; and this is one great ground of preference to this natural power. The same thing may be said of the action of a strong and light spring, which is therefore another very eligible first mover for machinery. The ancient artillerists had discovered this, and employed it in their warlike engines.

We must also attend to the nature of the resistance which the work to be performed opposes to the motion of our machine. Sometimes the work opposes not a simple obstruction, but a real resistance or re-action, which, if applied alone to the machine, would cause it to move the contrary way. This always obtains in cases where a heavy body is to be raised, where a spring is to be compressed, and in some other cases. Very often, however, there is no such contrary action. A flour-mill, a saw-mill, a boring-mill, and many such engines, exhibit no reaction of this kind.

But although such machines, when at rest or not impelled by the first mover, sustain no pressure in the opposite direction, yet they will not acquire any motion whatever; unless they be impelled by a power of a certain determinate intensity. Thus, in a saw-mill, a certain force must be impressed on the teeth of the saw, that the cohesion of the fibres of the timber may be overcome. This requires that a certain force, determined by the proportions of the machine, be impressed on the impelled point. If this, and no more, be applied there, a force will be excited at the teeth of the saw which will *balance* the cohesion of the wood, but will not *overcome it*. The machine will continue at rest, and no work will be performed. Any addition of force at the impelled point will occasion an addition to the force excited in the teeth of the saw. The cohesion will be overcome, the machine will move, and work will be performed. It is only this *addition* to the impelling power that gives motion to the machine; the rest being expended merely in balancing the cohesion of the woody fibres. While, therefore, the machine is in motion, performing work, we must consider it as actuated by a force impressed on the impelled point by the natural power, and by another acting at the working-point, furnished by, or derived from, the resistance of the work.

Again: It not unfrequently happens that there is not even any such resistance or obstruction excited at the working-point of the machine; the whole resistance (if we can with propriety give it that name) arises from the necessity of giving motion to a quantity of inert and inactive matter. This happens in urging round a heavy fly, as in the coining-press, in the punching-engine, in drawing a body along a horizontal plane without friction, and a few similar cases. Here the smallest force whatever, applied at the impelled point, will begin motion in the machine; and the *whole* force so applied is consumed in this service. Such cases are rare as the ultimate performance of a machine; but occasionally, and for a farther purpose, they frequently occur; and it is necessary to consider them, because there are many of the most important applications of machinery where a very considerable part of the force is expended in this part of the general task.

Such are the chief circumstances of distinction among the mechanical powers of nature which must be attended to, in order to know the motion and performance of a machine. These never occur in the statical consideration of the machine, but here they are of chief importance.

86. But farther: The action of the moving power is transferred to the working-point through the parts of a machine, which are material, inert, and heavy. Or, to describe it more accurately, before the necessary force can be excited at the working-point of the machine, the various connecting forces must be exerted in the different parts of the machine; and, in order that the working-point may follow out the impression already made, all the connecting parts or limbs of the machine must *be moved* in different directions, and with different velocities. Force is necessary for thus changing the state of all this matter, and frequently a very considerable force. Time must also elapse before all this can be accomplished. This often consumes, and really wastes, a great part of the impelling power. Thus, in a crane worked by men walking in a wheel, it acquires motion by slow degrees; because, in order to give sufficient room for the action of the number of men or cattle that are necessary, a very capacious wheel must be employed, containing a great quantity of inert matter. All of this must be put in motion by a very moderate preponderance of the men. It accelerates slowly, and the load is raised. When it has attained the required height, all this matter, now in considerable motion, must be stopped. This cannot be done in an instant with a jolt, which would be very inconvenient, and even hurtful; it is therefore brought to rest gradually. This also consumes time: nay, the wheel

must get a motion in the contrary direction, that the load may be lowered into the cart or lighter. This can only be accomplished by degrees. Then the tackle must be lowered down again for another load, which also must be done gradually. All this wastes a great deal both of time and of force, and renders a walking-wheel a very improper form for the first mover of a crane, or any machine whose use requires such frequent changes of motion. The same thing obtains, although in a lower degree, in the steam-engine, where the great beam and pump-rods, sometimes weighing very many tons, must be made to acquire a very brisk motion in opposite directions, twice in every working-stroke. It obtains, in a greater or a less degree, in all engines which have a reciprocating motion in any of their parts. Pump-mills are, of necessity, subjected to this inconvenience. In the famous engine at Marly, $\frac{19}{20}$ of the whole moving power of some of the water-wheels is employed in giving a reciprocating motion to a set of rods and chains, which extend from the wheels to a cistern about three-fourths of a mile distant, where they work a set of pumps. This engine is, by such injudicious construction, a monument of magnificence, and the struggle of ignorance with the unchangeable laws of nature. In machines, all the parts of which continue the direction of their motions unchanged, the inertia of a great mass of matter does no harm, but, on the contrary, contributes to the steadiness of the motion, in spite of small inequalities of power or resistance, or unavoidable irregularities of force in the interior parts. But in all reciprocations, it is highly prejudicial to the performance; and therefore constructions which admit such reciprocation without necessity, are avoided by all intelligent engineers. The mere copying artist, indeed, who derives all his knowledge from the common treatises of mechanics, will never suspect such imperfections, because they do not occur in the statical consideration of machines.

87. Lastly, no machine can move without a mutual rubbing of its parts at all points of communication; such as

the teeth of wheel-work, the wipers and lifts, and the gudgeons of its different axes. In many machines, the ultimate task performed by the working-point is either friction, or very much resembles it. This is the case in polishing-mills, grinding-mills, nay, in boring-mills, saw-mills, and others. A knowledge of friction, in all its varieties, seems therefore absolutely necessary, even for a moderate acquaintance with the principles of machinery. This is a very abstruse subject; and although a good deal of attention has been paid to it by some ingenious men, we do not think that a great deal has been added to our knowledge of it; nor do the experiments which have been made seem to us well calculated to lead us to a distinct knowledge of its nature and modifications. It has been considered chiefly with a view to diminish it as much as possible in the communicating parts of machinery, and to obtain some general rules for ascertaining the quantity of what unavoidably remains. Mr Amontons, of the Royal Academy of Sciences at Paris, gave us, about the beginning of this century, the chief information that we have on the subject. He discovered that the obstruction which it gave to motion was very nearly proportional to the force by which the rubbing surfaces are pressed together. Thus he found that a smooth oaken board, laid on another smooth board of the same wood, requires a force nearly equal to one-third of what presses the surfaces together. Different substances required different proportions.

88. He also found, that neither the extent of the rubbing surfaces, nor the velocity of the motion, made any considerable variation on the obstruction to motion. These were curious and unexpected results. Subsequent observations have made several corrections necessary in all these propositions;* but since the deviations from Mr Amonton's

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* The most recent and valuable experiments on the subject of friction were made by the celebrated Coulomb. He found that the friction increased with the time during which the rubbing surfaces were in contact; and that it in-

rules are not very considerable, at least in the cases which occur in this general consideration of machines, we shall make use of it in the mean time. It gives us a very easy method of estimating the effect of friction on machines. It is a certain proportion of the mutual pressure of the rubbing surfaces, and therefore must vary in the same proportion with this pressure. Now, we learn from the principles of statics, that whatever pressures are exerted on the impelled and working-point of the machine, all the pressures on its different parts have the same constant proportion to these, and vary as these vary: Therefore the whole friction of the machine varies in the same proportion. But farther, since it is found that the friction does not sensibly change with the velocity, the force which is just sufficient to overcome the friction, and put the loaded machine in motion, must be very nearly the same with the force expended in overcoming the friction while the machine is moving with any velocity whatever, and performing work. Therefore, if we deduct from the force which just puts the loaded machine in motion, that part of it which balances the reaction of the impelled point occasioned by the resistance of the work, or which balances the resistance of the work, the remainder is the part of the impelling power which is employed in overcoming the friction. If, indeed, the actual re-

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creased also with the velocity, the friction being nearly in an arithmetical progression, while the velocity was in a geometrical progression. It appeared in general that friction was proportional to the force with which the rubbing surfaces were pressed together, and was, for the most part, equal to between ½ and ¼ of that force. When oak rubbed against pine, it was $\frac{1}{1.54}$. When pine rubbed against pine, it was $\frac{1}{1.79}$, and oak against oak $\frac{1}{2.46}$. When iron rubbed against brass the friction was least, and was, in that case, ¼ of the force of pressure. A full account of Coulomb's experiments will be found in the *Mémoires des Savans Etrangers*, tom. x.; and a popular abstract of them in Ferguson's Lectures, vol. ii. p. 339.—Ed.

sisting pressure of the work varies with the velocity of the working-point, all the pressures, and all the frictions in the different communicating parts of the machine, vary in the same proportion. But the law of this variation of working resistance being known, the friction is again ascertained.

We can now state the dynamical equilibrium of forces in the working machine in two ways. We may either consider the efficient impelling power as diminished by all that portion which is expended in overcoming the friction, and which only prepares the machine for performing work, or we may consider the impelling power as entire, and the work as increased by the friction of the machine; that is, we may suppose the machine without friction, and that it is loaded with a quantity of additional resistance acting at the working-point. Either of these methods will give the same result, and each has its advantages. We adopted the last method in the slight view which we took of this subject in the article ROTATION, and shall therefore use it here.

Supposing now this previous knowledge of all these variable circumstances which affect the motion of machines of the rotative kind, so that, for any momentary position of it while performing work, we know what are the precise pressures acting at the impelled and working-points, and the construction of the machine, on which depend the friction, and the momentum of its inertia (expressed in the article ROTATION by $\int p\, r^2$); we are now in a condition to determine its motion, or at least its momentary acceleration, competent to that position. Therefore,

89. Let there be a rotative machine, so constructed, that while it is performing work, the velocity of its impelled point is to that of its working-point as m to n. It is easy to demonstrate, from the common principles of statics, that if a simple wheel and axle be substituted for it, having the radius of the wheel to that of the axle in the same proportion [illegible] to n, and having the same momentum of friction and

inertia, and actuated by the same pressures at the impelled and working-points, then the velocities of these points will be precisely the same as in the given machine.

Let *p* represent the intensity (which may be measured by pounds weight) of the pressure exerted in the moment at the impelled point; and *r* express the pressure exerted at the working-point by the resistance opposed by the work that is then performing. This may arise from the weight of a body to be raised, from the cohesion of timber to be sawed, &c. Any of these resistances may also be measured by pounds weight; because we know that a certain number of pounds hung on the saw of a saw-mill, will just overcome this cohesion, or overcome it with any degree of superiority. Therefore the impelling power *p*, and the resistance *r*, however differing in kind, may be compared as mere pressures.

Let *x* represent the quantity of inert matter which must be urged by the impelling power *p*, with the same velocity as the impelled point, in order that this pressure *p* may really continue to be exerted on that point. Thus, if the impelling power is a quantity of water in the bucket of an overshot wheel, acting by its weight, this weight cannot impel the wheel except by impelling the water. In this way, *x* may be considered as representing the inertia of the impelling power, while *p* represents its pressure on the machine. In like manner, let *y* represent the quantity of external inert matter which is really moved with the velocity of the working-point in the execution of the task performed by the machine.

Whatever be the momentum of the inertia of the machine, we can always ascertain what quantity of matter, attached to the impelled point, or the working-point of the wheel and axle, will require the same force to give the wheel the same angular motion; that is, which shall have the same momentum of inertia. Let the quantity *a*, attached to the working-point, give this momentum of inertia $a\,n^2$.

Lastly, supposing that the wheel and axle have no friction, let f be such a resistance, that, if applied to the working-point, it shall give the same obstruction as the friction of the machine, or require the same force at the impelled point to overcome it.

90. These things being thus established, the angular velocity of the wheel and axle, that is, the number of turns, or the portion of a turn, which it will make in a given time, will be proportional to the fraction

$$\frac{p.m. - \overline{r+f}\,n}{x m^2 + a + y n^2}. \quad \text{(I.)—See R}\text{OTATION.}$$

91. Since the whole turns together, the velocities of the different points are as their distances from the axis, and may be expressed by multiplying the common angular velocity by these distances. Therefore the above formula, multiplied by m or n, will give the velocity of the impelled or of the working-point. Consequently,

$$\text{Velocity of impelled point} = \frac{p m^2 - \overline{r+f}\,m n}{x m^2 + a + y n^2}. \quad \text{(II.)}$$

$$\text{Velocity of working-point} = \frac{p m n - \overline{r+f}\,n^2}{x m^2 + a + y n^2}. \quad \text{(III.)}$$

In order to obtain a clear conception of these velocities, we must compare them with motions with which we are well acquainted. The proposition being universally true, we may take a case where gravity is the sole power and resistance; where, for example, p and r are the weights of the water in the bucket of a wheel, and in the tube that is raised by it. In this case, $p = x$, and $r = y$. We may also, for greater simplicity, suppose the machine without inertia and friction. The velocity of p is now $\frac{p m^2 - r m n}{p m^2 + r n^2}$.

92. Let g be the velocity which gravity generates in a second. Then it will generate the velocity $g\dot{t}$ in the moment $\dot{t}$. Let v be the velocity generated during this moment in p, connected as it is with the wheel and axle, and with r. This connection produces a change of condition $= g\dot{t} - \dot{v}$. For, had it fallen freely, it would have acqui-

red the velocity $g\dot{t}$, whereas it only acquires the velocity $\dot{v}$. In like manner, had r fallen freely, it would have acquired the velocity $g\dot{t}$. But, instead of this, it is raised with the velocity $\frac{n}{m}\dot{v}$. The change on it is therefore $= g\dot{t} + \frac{n}{m}\dot{v}$. These changes of mechanical condition arise from their connection with the corporeal machine. Their pressures on it bring into action its connecting forces, and each of the two external forces is in immediate equilibrium with the force exerted by the other. The force excited at the impelling point, by r acting at the working-point, may be called the momentum or energy of r. These energies are precisely competent to the production of the changes which they really produce, and must therefore be conceived as having the same proportions. They are therefore equal and opposite, by the general laws *observed* in all actions of tangible matter; that is, they are such as balance each other. Thus, and only thus, the remaining motions are what we observe them to be.

$$\text{That is, } p \times \overline{g\dot{t} - \dot{v}} \times m = r \times \overline{g\dot{t} + \frac{n}{m}\dot{v}} \times n$$

$$\text{Or } pmg\dot{t} - pm\dot{v} = rng\dot{t} + r\frac{n^2}{m}\dot{v}$$

$$\text{Or } pm^2g\dot{t} - pm^2\dot{v} = rmng\dot{t} + rn^2\dot{v}$$

$$\text{Or } \overline{pm^2 - rmn} \times g\dot{t} = \overline{pm^2 + rn^2} \times \dot{v}$$

That is, $pm^2 + rn^2 : pm^2 - rmn = g\dot{t} : \dot{v}$

That is, *the denominator of the fraction, expressing the velocity of the impelled point, is to the numerator, as the velocity which a heavy body would acquire in the moment* $\dot{t}$, *by falling freely, is to the velocity which the impelled point acquires in that moment.* The same thing is true of the velocity of the working-point.

This reasoning suffers no change from the more complicated nature of the general proposition. Here the impelling power is still p, but the matter to be accelerated by it at the working-point is $a + y$, while its reaction, diminishing the impelling power, is only r. We have only to consider, in this case, the velocity with which $a + y$ would fall freely when impelled, not by $a + y$, but only by r. The result would be the same; $g\,\dot{t}$ would still be to $\dot{v}$ as the denominator of the same fraction to its numerator.

Thus have we discovered the momentary acceleration of our machine. It is evident, that if the pressures p and r, and the friction and inertia of the machine, and the external matter, continue the same, the acceleration will continue the same; the motion of rotation will be uniformly accelerated, and $p\,m^2 + \overline{a + y}\,n^2$ will be to $p\,m^2 - \overline{r + f}\,m\,n$ as the space s, through which a heavy body would fall in any given time t, is to the space through which the impelled point will really have moved in the same time. In like manner, the space through which the working-point moves in the same time is $= \frac{p\,m\,n - \overline{r+f}\,n^2}{p\,m^2 + \overline{a + y}\,n^2}\,s$.

Thus are the motions of the working-machine determined. We may illustrate it by a very simple example. Suppose a weight p of five pounds, descending from a pulley, and dragging up another weight r of three pounds on the other side. m and n are equal, and each may be called 1. The formula becomes $\frac{p - r}{p + r}s$, or $\frac{5-3}{5+3}s$, or $\frac{2}{8}s = \frac{1}{4}s$. Therefore, in a second, the weight p will descend ¼th of 16 feet, or 4 feet; and will acquire the velocity of 8 feet per second.

93. Having obtained a knowledge of the velocity of every point of the machine, we can easily ascertain its performance. This depends on a combination of the quantity of resistance that is overcome at the working-point, and the velocity with which it is overcome. Thus, in raising water, it depends on the quantity (proportional to the weight) of water in the bucket or pump, and the velocity with which it is lifted up. This will be had by multiplying the third

formula by r, or by $r\ g\ \dot{t}$, or by $r\ s$. Therefore we obtain this expression,

$$\text{Work done} = \frac{p\ m\ r\ n - \overline{r+f}\ r\ n^2}{p\ m^2 + \overline{a+y}\ n^2}\ g\ \dot{t}. \quad \text{(IV.)}$$

Such is the general expression of the momentary performance of the machine, including every circumstance which can affect it. But a variation of those circumstances produces great changes in the results. These must be distinctly noticed.

Cor. 1. If $p\ m\ r\ n$ be equal to $\overline{r+f}\ r\ n^2$, there will be no work done, because the numerator of the fraction is annihilated. There is then no unbalanced force, and the natural power is only able to balance the pressure propagated from the working-point to the impelled point.

2. In like manner, if $n = o$, no work is done although the machine turns round. The working-point has no motion. For the same reason, if m be infinitely great, although there is a great prevalence of impelling momentum, there will not be any sensible performance during a finite time. For the velocity which p can impress is a finite quantity, and the impelled point cannot move faster than x would be moved by it if detached from the machine. Now, when the infinitely remote impelled point is moved through any finite space, the motion of the working-point must be infinitely less, or nothing, and no work will be done.

Remark. We see that there are two values of n, viz. o, and $m \times \frac{p}{r}$, which give no performance. But in all other proportions of m and n, some work is done. Therefore, as we gradually vary the proportion of m to n, we obtain a series of values expressing the performance, which must gradually increase from nothing, and then decrease to nothing. There must, therefore, be some proportion of m to n, depending on the proportion of p to $\overline{r+f}$, and of x to $\overline{a+y}$, which will give the greatest possible value of the performance. And, on the other hand, if the proportion of m to n be already determined by the construction of the machine already erected, there must be some proportion of p to $\overline{r+f}$, and of x to $\overline{a+y}$, by which the greatest performance of the machine may be ensured. It is evident that the determination of these two proportions is of the utmost importance to the improvement of machines. The well-informed reader will pardon us for endeavouring to make this appear more forcibly to those who are less instructed, by means of some very simple examples of the first principle.

Suppose that we have a stream of water affording three tons per minute, and that we want to drain a pit which receives one ton per minute, and that this is to be done by a wheel and axle? We wish to know the best proportion of their diameters m and n. Let m be taken $= 6$; and suppose,

1. That $n = 5$.

Then $\frac{p\,m\,r\,n - r^2\,n^2}{p\,m^2 + r\,n^2} = \frac{3.6.1.5 - 1.25}{3.36 + 1.36}, = \frac{65}{133}, = 0,4887$

2. Let n be $= 6$. The formula is $= 0,5$.

3. Let $n = 7$. The formula is $= 0,49045$. Hence we find, that the performance is greater when n is 6, than when it is either 5 or 7.

As an example of the second principle, suppose the machine a simple pulley, and let p be 10.

1. Let r be $= 3$. The formula is $\frac{10 \times 3 - 9}{10+3}, = \frac{21}{13}, =$ 1,6154.

2. Let r be $= 4$. The formula is $= \frac{10 \times 4 - 16}{10+4}, = \frac{24}{14}, = 1,7143$.

3. Let r be $= 5$. The formula is $= \frac{10 \times 5 - 25}{10+5}, = \frac{25}{15}, = 1,6666$. Here it appears that more work is done when r is 4, than when it is 5 or 3.

It must, therefore, be allowed to be one of the most important problems in practical mechanics to determine that construction by which a given power shall overcome a given resistance with the greatest advantage, and the proportion of work which should be given to a machine already constructed so as to gain a similar end.

94. I. The general determination of the first question has but little difficulty. We must consider n as the variable magnitude in the formula $\frac{pmrn - \overline{r+f}\,rn^2}{pm^2 + \overline{a+y}\,n^2}$, which expresses the work done, and find its value when the formula is a maximum. Taking this method, we shall find that the formula IV. is a maximum when n is

$$= m\,\frac{\sqrt{x^2(r+f)^2 + p^2x(a+y)}\,| - x(r+f)}{p(a+y)}.$$

This expression of the performance, in its best state, appears pretty complex; but it becomes much more simple in all the particular applications of it, as the circumstances of the case occur in practice.

We have obtained a value of n expressed in parts of m. If we substitute this for n in the third formula, we obtain the greatest velocity with which the resistance r, connected with the inertia y, can be overcome by the power p, connected with the inertia x, by the intervention of a machine, whose momentum of inertia and friction are an^2 and fn. This is $= \frac{r+f}{2\,\overline{a+y}} \times \left(\sqrt{\frac{p^2\,\overline{a+y}}{\overline{r+f}^2\,x} + 1}\,\Big| - 1\right) g\,t$. This expresses the velocity of the working-point in feet per second, and therefore the actual performance of the machine.

But the proper proportion of m to n, ascertained by this process, varies exceedingly, according to the nature both of the impelling power, and of the work to be performed by the machine.

1. It frequently happens that the work exerts no contrary strain on the machine, and consists merely in im-

pelling a body which resists only by its inertia. This is the case in urging round a millstone or a heavy fly; in urging a body along a horizontal plane, &c. In this case r does not enter into the formula, which now becomes $m \times \frac{\sqrt{x^2 f^2 + p^2 x (a+y)} - x f}{p(a+y)}$. If the friction be insignificant we may take $n = m \sqrt{\frac{p^2 x (a+y)}{p^2 (a+y)^2}}$, $= m \sqrt{\frac{x}{a+y}}$. The velocity of the working-point is nearly $= \frac{p}{2\sqrt{x\,\overline{a+y}}}$. In this case, it will be found that the velocity acquired at the end of a given time will be nearly in the proportion of the power applied to the machine.

2. On the other hand, and more frequently, the inertia of the external matter which must be moved in performing the work, need not be regarded. Thus, in the grinding of grain, sawing of timber, boring of cylinders, &c. the quantity of motion communicated to the flour, to the saw-dust, &c. is too insignificant to be taken into the account. In this case, y vanishes from the formula, which becomes extremely simple when the friction and inertia of the machine are inconsiderable. We shall not be far from the truth if we make m to n as $2\,r$ to p, or $n = m \times \frac{p}{2\,\overline{r+f}}$. In this case, the velocity of the working-point is $\frac{p^2}{4\,x(r+f) + \frac{a\,p^2}{4(r+f)}}$. But it is rare that machines of this kind have a small inertia. They are generally very ponderous and powerful; and the force which is necessary for generating even a very moderate motion in the unloaded machine, (that is, unloaded with any work,) bears a great proportion to the force necessary for overcoming the resistance opposed by the work. The formula must, therefore, be used in all the terms, be-

cause a is joined with y. It would have been simpler in this particular, had a been joined with x in the expression of the angular velocity.

3. In some cases we need not attend to the inertia of the power, as in the steam-engine. In this case, if taken strictly, n appears to have no value, because x is a factor of every term of the numerator. But the formula gives this general indication, that the more insignificant the inertia of the moving power is supposed, the larger should m be in proportion to n; provided always, that the impelling power is not, by its nature, greatly diminished, by giving so great a velocity to the impelled point. This circumstance will be particularly considered afterwards.

4. If the inertia of the power and the resistance be proportional to their pressures, as when the impelling power is water lying in the buckets of an overshot wheel, and the work is the raising of water, minerals, or other heavy body, acting *only* by its weight; then p and r may be substituted for x and y, and the formula expressing the value of n, when the performance is a maximum, becomes

$$n = m\,\frac{\sqrt{p^2 \times \overline{r+f}^2 + p^3 \times \overline{a+r}} - p \times \overline{r+f}}{p \times a + r}.$$

If, in this case, the inertia and friction of the machine may be disregarded, as may often be done in pulleys, we have

$$n = m\sqrt{\frac{p}{r} + 1} - 1.$$

If we make m the unit of the radii, and r the unit of force, we have

$$n = \sqrt{p+1} - 1, \text{ in parts of } m = 1.$$

$$\text{Or, making } p = 1, \text{ we have } n = \sqrt{\frac{1}{r} + 1} - 1.$$

These very simple expressions are of considerable use, even in cases where the inertia of the machine is very considerable, provided that it have no reciprocating motions. A simple wheel and axle, or a train of good wheel-work, have very moderate friction. The general results, therefore, which even very unlettered readers can deduce from these simple formulæ, will give notions that are useful in the cases which they cannot so thoroughly comprehend. Some service of this kind may be derived from the following little table of the best proportions of *m* to *n*, corresponding to the proportions of the power furnished to the engineer, and the resistance which must be overcome by it. The quantity *r* is always = 10, and *m* = 1.*

p	*n*	*p*	*n*
1	0,048809	20	0,732051
2	0,095445	21	0,760682
3	0,140175	22	0,788854
4	0 183216	23	0,816590
5	0,224745	24	0,843900
6	0,264911	25	0,870800
7	0,303841	26	0,897300
8	0,341641	27	0,923500
9	0,378405	28	0,949400
10	0,414214	29	0,974800

This must suffice for a very general view of the first problem.

95. II. The next question is not less momentous, namely, to determine for a machine of a given construction that proportion of the resistance at the working-point to the impelling power which will insure the greatest performance of the machine; that is, the proportion of *m* to *n* being given, to find the best proportion of *p* to *r*.

This is a much more complicated problem than the other; for here we have to attend to the variations both of the pressures *p* and *r*, and also of the external matters *x* and *y*, which are generally connected with them. It will not be sufficient, therefore, to treat the question by the usual fluxionary process for determining the maximum, in which r is considered as the only varying quantity. We must, in this cursory discussion, rest satisfied with a comprehension of the circumstances which most generally prevail in practice.

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* We have recomputed this table from the formula, extending the numbers two decimal places, and adding the values of *n* for several intermediate values of *p*.—ED.

It must either happen, that when r changes, there is no change (that is, of moment) in the mass of external matter which must be moved in performing the work, or that there is also a change in this circumstance. If no change happens, the denominator of the fourth formula, expressing the performance, remains the same; and then the formula attains a maximum when the numerator $p\,r\,m\,n - \overline{r+f}\,r\,n^2$ is a maximum. Also, we may include f without complicating the process, by the consideration, that f is always in nearly the same ratio to r; and therefore $r+f$ may be considered as a certain multiple of r, such as $b\,r$. We may, therefore, omit f in the fluxionary equations for obtaining the maximum, and then, in computing the performance, divide the whole by b. Thus if the whole friction be $\frac{1}{20}$th of the resisting pressure r, we have $r+f = \frac{21}{20}$ of r, and $b = \frac{21}{20}$. Having ascertained the best value for r, we put this in its place in the fourth formula, and take $\frac{20}{21}$ of this for the performance. This will never differ much from the truth.

This process gives us $p\,m\,n = 2\,n^2\,r$, and $r = \frac{p\,m\,n}{2\,n^2}$, $= \frac{p\,m}{2\,n}$; and if we farther simplify the process, by making $p = 1$, and $m = 1$, we have $r = \frac{1}{2\,n}$; a most simple expression, directing us to make the resistance one-half of what would balance the impelling power by the intervention of the machine.

This will evidently apply to many very important cases, namely, to all those in which the matter put in motion by the working-point is but trifling.

But it also happens in many important cases, that the change is at least equally considerable in the inertia of the work. In this case it is very difficult to obtain a general solution. But we can hardly imagine such a change, without supposing that the inertia of the work varies in the same proportion as the pressure excited by it at the working-point of the machine; for since r continues the same in kind, it can rarely change but by a proportional change of the matter with which it is connected. Yet some very important cases occur where this does not happen. Such is a machine which forces water along a long main pipe. The resistance to motion and the quantity of water do not follow nearly the same ratio. But in the cases in which this ratio is observed, we may represent y by any multiple $b\,r$ of r, which the case in hand gives us; b being a number, integer, or fractional. In the farther treatment of this case, we think it more convenient to free r from all other combinations; and instead of supposing the force f (which we made equivalent with the friction of the machine) to be applied at the working-point, we may apply it at the impelled point, making the effective power $q = p - f$. For the same reasons, instead of making the momentum of the machine's inertia $= a\,n^2$, we may make it $a\,m^2$, and make $a + x = z$. Now, supposing q, or $p - f$, $= 1$, and also $m = 1$, our formula expressing the performance becomes $\frac{r\,n - r^2\,n^2}{z + b\,r\,n^2}$. This is a maximum when $r = \frac{\sqrt{z^2 + z\,b\,n} - z}{b\,n^2}$.

Cor. 1. If the inertia of the work is always equal to its pressure, as when the work consists wholly in raising a weight, such as drawing water, &c. then $b = 1$, and the formula for the maximum performance becomes

$$r = \frac{\sqrt{zn + z^2} - z}{n^2}.$$

2. If the inertia of the impelling power is also the same with its pressure, and if we may neglect the inertia and friction of the machine, the formula becomes

$$r = \frac{\sqrt{n + 1} - 1}{n^2}.$$

Example. Let the machine be a common pulley, so that the radii m and n are equal, and therefore $n = 1$. Then, $r = \frac{\sqrt{1 + 1} - 1}{1}$, $= \sqrt{2} - 1$, $= 0,4142$, &c. more than $\frac{2}{5}$ths of what would balance it.

Here follows a series of the best values of r, corresponding to different values of n. m and p are each $= 1$. The numbers in the last column have the same proportion to 1 which r has to the resistance which will balance p.*

$n = \frac{1}{4}$	$r =$ 1,8885	0,4724 to 1
$\frac{1}{3}$	1,3928	0,4639
$\frac{1}{2}$	0,8986	0,4493
1	0,4142	0,4142
2	0,1830	0,3660
3	0,1111	0,3333
4	0,0772	0,3088
5	0,0580	0,2900
6	0,0451	0,2742
7	0,03731	0,26117
8	0,03125	0,25000
9	0,02669	0,24021
10	0,02317	0,23170
11	0,02037	0,22407
12	0,01809	0,21708

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* The above table has been extended to values of n from 5 to 12 inclusive.—ED.

From what has now been established, we see, with sufficient evidence, the importance of the higher mathematics to the science of mechanics. If the velocities of the impelled and working points of an engine are not properly adjusted to the pressures, the inertia, and the friction of the machine, we do not derive all the advantages which we might from our situation. Hence also we learn the falsity of the maxim which has been received as well founded, that the augmentation of intensity of any force, by applying it to the long arm of a lever, is always fully compensated by a loss of time; or, as it is usually expressed, "what we gain by a machine in force we lose in time." If the proportion of m to n is well chosen, we shall find that the work done, when it resists by its inertia only, increases nearly in the proportion of the power employed; whereas, when the inertia of the work is but a small part of the resistance, it increases nearly in the duplicate ratio of the power employed.

It was remarked, in the setting out in the present problem, that the formulæ do not immediately express the velocity of any point of the machine, but its momentary acceleration. But this is enough for our purpose; because, when the momentary acceleration is a maximum, the velocity acquired, and the space described, in any given time, is also a maximum. We also shewed how the real velocities, and the spaces described, may be ascertained in known measures. We may say in general, that if g represent the pressure of gravity on any mass of matter w, then $\frac{g}{w}$ is to $\frac{p m n - \overline{r + f}\, n}{a m^2 + \overline{a + y}\, n^2}$ as 16 feet to the space described in a second by the working-point in a second, or as 32 feet per second is to the velocity acquired in that time.

96. A remark now remains to be made, which is of the greatest consequence, and gives an unexpected turn to the whole of the preceding doctrines. It appears, from all that has been said, that the motion of a machine must be uniformly accelerated, and that any point will describe spaces

proportional to the squares of the times; for while the pressures, friction, and momentum of inertia remain the same, the momentary acceleration must also be invariable. But this seems contrary to all experience. Such machines as are properly constructed, and work without jolts, are observed to quicken their pace for a few seconds after starting; but all of them, in a very moderate time, acquire a motion that is sensibly uniform. Is our theory erroneous, or what are the circumstances which remain to be considered, in order to make it agree with observation? The science of machines is imperfect, till we have explained the causes of this deviation from the theory of uniform acceleration.

The causes are various.

1. In some cases, every increase of velocity of the machine produces an increase of friction in all its communicating parts. By these means, the accelerating force, which is $p m - \overline{r + f} n$, or $\overline{p - f} m - r n$, is diminished, and consequently the acceleration is diminished. But it seldom happens that friction takes away or employs the whole accelerating force. We are not yet well instructed in the nature of friction. Most of the kinds of friction which obtain in the communicating parts of machines, are such as do not sensibly increase by an increase of velocity; some of them really diminish. Yet even the most accurately constructed machines, unloaded with work, attain a motion that is sensibly uniform. If we take off the pallets from a pendulum clock, and allow it to run down amain, it accelerates for a while, but in a very moderate time it acquires an uniform motion. So does a common kitchen jack. These two machines seem to bid the fairest of any for an uniformly accelerated motion; for their impelling power acts with the utmost uniformity. There is something yet unexplained in the nature of friction, which takes away some of this acceleration.

2. But the chief cause of its cessation in these two instances, and others *of very rapid motion*, is the resistance of the air.

This arises from the motion which is communicated to the air displaced by the swift moving parts of the machine. At first it is very small; but it increases nearly in the duplicate ratio of the velocity (see Resistance *of Fluids.*) Thus r increases continually; and, in a certain state of motion, $\overline{r+f}\,n$ becomes equal to $p\,m$. Whenever this happens, the accelerating power is at an end. The acceleration also ceases; and the machine is in a state of dynamical equilibrium; not at rest, but moving uniformly, and performing work.

3. Still, however, this is not one of the general causes of the uniform motion attained by working engines. Rarely is the motion of their parts so rapid, as to occasion any great resistance from the air. But in the most frequent employments of machines, every increase of velocity is accompanied by an increase of resistance from the work performed. This occurs at once to the imagination; and few persons think of enquiring farther for a reason. But there is, perhaps, no part of mechanics that is more imperfectly understood, even in our present improved state of mechanical science. In many kinds of work, it is very difficult to state what increase of labour is required in order to perform the work with twice or thrice the speed. In grinding corn, for instance, we are almost entirely ignorant of this matter. It is very certain, that twice the force is not necessary for making the mill grind twice as fast, nor even for making it grind twice as much grain equally well. It is not easy to bring this operation under mathematical treatment; but we have considered it with some attention, and we imagine that a very great improvement may still be made in the construction of grist mills, founded on the law of variation of the resistance to the operation of grinding, and a scientific adjustment of m to n, in consequence of our knowledge of this law. We may make a similar observation on many other kinds of work performed by machines. In none of those works where the inertia of the work is inconsiderable, are we well acquainted with the real mechanical process in

performing it. This is the case in sawing-mills, boring-mills, rolling-mills, slitting-mills, and many others, where the work consists in overcoming the strong cohesion of a small quantity of matter. In sawing timber, (which is the most easily understood of all these operations,) if the saw move with a double velocity, it is very difficult to say how much the actual resisting pressure on the teeth of the saw is increased. Twice the number of fibres are neccssarily torn asunder during the same time, because the same number are torn by one descent of the saw, and it makes that stroke in half the time. But it is very uncertain whether the resistance is double on this account; because if each fibre be supposed to have the same tenacity in both cases, it resists with this tenacity only for half the time. The parts of bodies resist a similar change of condition in different manners; and there is another difference in their resistance of different changes—the resistance of red-hot iron under the roller may vary at a very different rate from that of its resistance to the cutting tool. The resistance of the spindles of a cotton-mill, arising partly from friction, partly from the inertia of the heaped bobins, and partly from the resistance of the air, is still more complicated, and it may be difficult to learn its law. The only case in which we can judge with some precision is, when the inertia of matter, or a constant pressure like that of gravity, constitutes the chief resistance. Thus in a mill employed to raise water by a chain of buckets, the resistance proceeds from the inertia only of the water. The buckets are moving with a certain velocity, and the lowest of them takes hold of a quantity of water lying at rest in the pit, and drags it into motion with its acquired velocity. The force required for generating this motion on the quiescent water must be double or triple, when the velocity that must be given to it is so. This absorbs the overplus of the impelling power, by which that power exceeds what is necessary for balancing the weight of the water contained in all the ascending buckets. This is a certain determinate quantity which does not change;

for in the same instant that a new bucket of water is forced into motion below, and its weight added to that of the ascending buckets, an equal bucket is emptied of its water at top. The ascending buckets require only to be balanced, and they then *continue* to ascend, with any velocity already acquired. While the machine moves slow, the motion impressed on the new bucket of water is not sufficient to absorb all the overplus of impelling power. The quantity not absorbed accelerates the machine, and the next bucket must produce more motion in the water which it takes up. This consumes more of the overplus. This goes on till no overplus of power is left, and the machine accelerates no more. The complete performance of the machine now is, that "a certain quantity of water, formerly at rest, is now moving with a certain velocity." Our engineers consider it differently; "as a certain *weight* of water lifted up." But while the machine is thus moving uniformly, it is really not doing so much as before; that is, it is not exerting such great pressures as before the motion was rendered uniform; for at that time there was a pressure at the working-point equal to the weight of all the water in the ascending buckets; and also an overplus of pressure, by which the whole was accelerated. In the state of uniform motion, the pressure is no more than just balances the weight of the ascending chain. We shall learn by and by how the pressures have been diminishing, although the mill has been accelerating; a thing that seems a paradox.

In this instance, then, we see clearly, why a machine must attain a uniform motion. A pumping machine gives us the same opportunity, but in a manner so different as to require explanation. The piston may be supposed at the very surface of the pit water, and the impelling power may be less than will support a column in the pipe as high as can be raised by the pressure of the atmosphere. Suppose the impelling power to be the water lying in the buckets of an overshot wheel. Let this water be laid into the buckets by a very small stream. It will fill the buckets very slowly;

and, as this gives them a preponderance, the mill loses its balance, the wheel begins to move, and the piston to rise, and the water to follow it. The water may be delivered on the wheel drop by drop; the piston will rise by insensible degrees, always standing still again as soon as the atmospheric pressure on it just balances the water on the wheel. The water in the rising-pipe is always a balance to the pressure of the atmosphere on the cistern; therefore the pressure of the atmosphere on the piston (which is the r in our formula) is equal to the weight of this water. Our pump-makers therefore (calling themselves engineers) say, that the weight of water in the pipe balances the water on the wheel. It does not balance it, nor is it raised by the wheel, but by the atmosphere; but it serves us at present for a measure of the power of the wheel. At last, all the buckets of the wheel are full, and the water is (for example) 25 feet high in the pipe. Now let the stream of water run its full quantity. It will only run over from bucket to bucket, and run off at the bottom of the wheel; but the mill will not move, and no work will be performed. (N. B. We are here excluding all impulse or stroke on the buckets, and supposing the water to act only by its weight.) But now let all be emptied again, and let the water be delivered on the wheel in its full quantity at the first. The wheel will immediately acquire a preponderancy, which will *greatly* exceed the first small pressure of the atmosphere on the piston. It will, therefore, accelerate the piston, overcoming the pressure of the air with great velocity. The piston rises fast; the water follows it by the pressure of the atmosphere; and when it attains the former utmost height, it attains it with a considerable velocity. If allowed to run off there, it will *continue* to run off with that velocity; because there is the same quantity of water pressing round the wheel as before, and therefore enough to balance the pressure of the atmosphere on the piston. The pressure of the same atmosphere on the water in the cistern, raised the water in the pipe with this velocity; therefore it will continue to do so, and the mill will deliver water by the pump with this velocity, although there is no more pressure acting on it than before, when the water ran to waste, doing no work whatever.

This mode of action is extremely different from the former example. The mill is not acting against the inertia y of the water to be moved, but against the pressure r of the atmosphere on the piston. The pressure of the same atmosphere on the cistern is employed against the inertia of the water in the pipe; and the use of the mill is to *give occasion*, by raising the piston, to the exertion of this atmospherical pressure, which is the real raiser of the water. The maxim of construction, and the proper adjustment of m to n in this case, are different from the former; and we should run the risk of making an imperfect engine were we to confound them.

We must mention another case of a pumping-mill, seemingly the same with this, but essentially different. Suppose the pipe of this pump to reach 30 feet below the surface of the pit-water, and that the piston is at the very bottom of it. Suppose also, that the wheel buckets, when filled with water, only enable it to *support* 25 feet of water in the rising pipe. Let the water be delivered into the wheel drop by drop. The wheel will gradually preponderate; the piston will gradually rise, lifting the water above it, sustaining a pressure of water which gradually increases. At last, the water in the pump is 25 feet higher than that in the cistern; the wheel is full and running to waste; but no work is performed. Let all be emptied, and now let the water come to the wheel in its full stream, but without impulse. The piston will lift the water briskly, bring it to 25 feet high with a considerable velocity, and the mill will now raise it with this velocity. In this example, the mill is the immediate agent in raising the water; but, in this case also, its ultimate office is not overcoming inertia, but overcoming pressure. It was the overplus of power only that was employed in overcoming inertia, while accelerating the water in the rising pipe, in order to give it the necessary velocity for a continued discharge.

97. These and similar examples shew the great difference between the statical and dynamical equilibrium of machines, and the necessity of a scientific attention by all who wish to improve practical mechanics. Without this, and even a pretty refined attention, we cannot see the connection between a copious supply of water to the bucket-wheel and a plentiful discharge by the pump. We believe, that the greatest part of those employed in erecting machines conceive it as owing to the greater weight of water impelling the wheel with greater force; but we see that there is no difference in the pressures on the mill at rest, and the mill doing its work steadily and uniformly, with any velocity, however great. Without keeping the notions of that part of the impelling power which supports distinct from that of the part which accelerates, we shall never have a clear conception of the operation of machines, or of mechanical power in general. We cannot derive all the advantages of our natural powers, without knowing how our machine employs the pressure excited by it at the working point; that is, without perceiving in what cases it is opposed to inertia, and in what to the mechanical properties of tangible matter. This only can inform us at what rate the resistance varies by a change of velocity; and when it happens that this augmentation, necessarily accompanied by an augmentation of all the frictions, and the resistance of the air, is in equilibrio with the whole of the impelling power, and all acceleration is at an end.

98. Lastly, another chief cause of the finally uniform motion of machines is, that, in most cases, an increase of velocity produces a real diminution of impelling power. We hardly know any exception to this besides the employment of *one* descending weight as a power or first mover. Most of the powers which we employ reside in bodies external to the machine; and these bodies must be put in motion, and

continued in that motion, in order to continue their pressure on the impelled point. Frequently a great part of the power is employed in giving this necessary motion to the external matter, and the remainder only is employed in pressing forward the machine. We mentioned a remarkable instance of this in the operation of threshing. Now, the power thus employed must increase in proportion to the motion required; that is, in proportion to the velocity of the impelled point; what remains, urging forward the machine, is therefore diminished. The acceleration is therefore diminished, and may cease. At *last* the actual pressure is so much diminished, that it is no more than what is necessary for overcoming the increased resistance of the work, the increased friction. The machine, therefore, accelerates no more, but moves uniformly.

This cause of the diminution of power by an increase of velocity, obtains in all cases where the strength of animals, of springs, the force of fired gunpowder, &c. is exerted. In some cases, the visible effect is not very considerable; as in the employment of a strong spring, the force of gunpowder, and a few others. In the action of animals, this defalcation of power is very great when the velocity is considerable. Nay, even in the action of gravity, although it acts as strongly on a body in rapid motion as on one at rest, yet when gravity is not the immediate agent, but acts by the intervention of a body in which it resides, the necessity of previously moving this body frequently diminishes the acceleration which it would otherwise produce. Thus, in an overshot-wheel, if the water be delivered into the bucket with a velocity (estimated in the direction of the part of the wheel into which it is delivered) less than that of the rim of the wheel, it must retard the motion; for it must be immediately dragged into that motion; that is, part of the accelerating overplus, already acting on the wheel, must be employed in accelerating this new bucket of water, and this must lessen the general acceleration of the machine. Hence we learn, that the water must be delivered on the wheel with a velocity that is at least not less than that of the wheel's motion.

The case in which we see this diminution of power on machines most distinctly is, when water or wind, acting by impulse alone, is our moving power. Since the mutual impulses of bodies depend entirely on their relative motions, it follows, that when the velocity of the impelled point is augmented, the impulsion, or effective pressure, must be diminished. Nay, this velocity may be so increased, that there shall be no relative motion, and therefore no impulsion. If the floats of an undershot wheel be moving with the velocity of the stream, they remain conjoined in their progress, but without any mutual action. Therefore, when an undershot wheel is set into a running water, the first impulsions are strong, and accelerate the wheel. This diminishes the next impulsion and acceleration: but the wheel is still impelled and accelerated; less and less in every succeeding moment, as it moves faster; by and by, the acceleration becomes insensible, and the wheel appears to attain a motion which is perfectly uniform. This requires a very long time, or rather it is never attained, and we only cannot discern the very small additions which are still made to the velocity. All this happens generally after a very moderate time, by reason of various other obstructions.

Animal action is subject to the same variation. We know that there is a certain rate at which a horse can run, exhausting or employing his whole strength. If he be made to drag any the smallest load after him, he must employ part of his force on it, and his speed will be checked. The more he is loaded with a draught, the slower he will run, still employing all his strength. The draught may be increased till he is reduced to a trot, to a walk, nay, till he is unable to draw it. Now, just inverting this process, we see that there is a certain strain which will sufficiently tire the horse without stirring from the spot, but which he could continue to exert for hours. This is greater than the load that he can crawl along with, employing his strength as much as would be prudent to continue from day to day. And, in like manner, every lesser draught has a corresponding rate, at which the horse, employing his whole working strength, can continue to draw at during the working hours of a day. At setting out, he pulls harder, and accelerates it. Following his pull, he walks faster, and therefore pulls less (because we are still supposing him to employ his whole working strength.) At last he attains that speed which occupies his whole strength in merely continuing the pull. Other animals act in a similar manner; and it becomes a general rule, that the pressure actually exerted on the impelled point of a machine diminishes as its velocity increases.

99. From the concurrence of so many facts, we perceive that we must be careful to distinguish between the quantity of power expended, and the quantity that is usefully employed, which must be measured solely by the pressure exerted on the machine. When a weight of five pounds is employed to drag up a weight of three pounds by means of a thread over a pully, it descends, with a motion uniformly accelerated, four feet in the first second. Mr Smeaton would call this an expenditure of a mechanical power 20. The weight three pounds is raised four feet. Mr Smeaton would call this a mechanical effect 12. Therefore the effect produced is not adequate to the power expended. But the fact is, that the pressure, strain, or mechanical power really exerted in this experiment, is neither five nor three pounds; the five-pound weight would have fallen 16 feet, but it falls only four. A force has therefore acted on it sufficient to make it describe 12 feet in a second, with a uniformly accelerated motion; for it has counteracted so much of its weight. The thread was strained with a force equal to 3$\frac{3}{4}$ pounds, or $\frac{3}{4}$ths of 5 pounds. In like manner, the 3-pound weight would have fallen 16 feet; but it was raised

4 feet. Here was a change precisely equal to the other. A force of 3¾ pounds, acting on a mass whose matter is only 3, will, in a second, cause it to describe 20 feet with a uniformly accelerated motion. Now, 5 × 12, and 3 × 20, give the same product 60. And thus we see, that the quantity of motion extinguished or produced, and not the product of the weight and height, is the true unequivocal measure of mechanical power really expended, or the mechanical effect really produced, and that these two are always equal and opposite. At the same time, Mr Smeaton's theorem merits the attention of engineers, because it generally measures the opportunities that we have for procuring the exertion of power. In some sense, Mr Smeaton may say, that the quantity of water multiplied by the height from which it descends in working our machines, is the measure of the power expended, because we must raise this quantity to the dam again, in order to have the same use of it. It is expended, but not employed; for the water, at leaving the wheel, is still able to do something.

100. It requires but little consideration to be sensible that the preceding account of the cessation of accelerated motion in our princpal machines, must introduce different maxims of construction from those which were expressly adapted to this acceleration; or rather, which proceeded on the erroneous supposition of the constancy of the impelling power and the resistance. The examination of this point has brought into view the fundamental principle of working machines, namely, the perfect equilibrium which takes place between the impelling power aud the simultaneous resistance. It may be expressed thus:

The force required for preserving a machine in uniform motion, with any velocity whatever, is that which is necessary for balancing the resistance then actually exerted on the working-point of the machine. We saw this distinctly in the instance of the two weights acting against each other by the intervention of a thread over a fixed pulley. It is equally true of every case of acting machinery: for if the force at the impelled point be greater than what balances the resistance acting at the same point, it must accelerate that point, and therefore accelerate the whole machine; and if the impelling force be less than this, the machine must immediately retard in its motion. When the machine has once acquired this degree of motion, every part of it will continue in its present state of motion, if only the two external forces are in equilibrio, but not otherwise. But when the pressure of the external power on the impelled point balances the resistance opposed by that point, it is, in fact, maintaining the equilibrium with the external power acting at the working-point; for this is the only way that external forces can be set in opposition to each other by the intervention of a body. The external forces are not in immediate equilibrio with each other, but each is in equilibrio with the force exerted by the point on which it acts. This force exerted by the point is a modification of the connecting forces of the body, all of which are brought into action by means of the actions of the external forces, and each is accompanied by a force precisely equal and opposite to it. Now, the principles of statics teach us the proportions of the external pressures which are thus set in equilibrium by the intervention of a body; and therefore teach us what proportion of power and resistance will keep a machine of a given construction in a state of uniform motion.

This proposition appears paradoxical, and contrary to common observation; for we find, that, in order to make a mill go faster, we must either diminish the resistance, or we must employ more men, or more water, or water moving with greater velocity, &c. But this arises from some of the causes already mentioned. Either the resistance of the work is greater when the machine is made to move faster, or the impulsion of the power is diminished, or both these changes obtain. Friction and resistance of air also come in for their share, &c. The actual pressure of a given quantity of the external power is diminished, and therefore more of it must be employed. When a weight is *uniformly* raised by a machine, the pressure exerted on it by the working-point is precisely equal to its weight, whatever be the velocity with which it rises. But, even in this simplest case, more natural power must be expended in order to raise it faster; because either more natural power must be employed to accelerate the external matter which is to press forward the impelled point, or the relative motion of the pressing matter will be diminished.

It is well known, that, in the employment of the mechanic powers, whether in their state of greatest simplicity, or any how combined in a complicated machine, if the machine be put in motion, the velocities of the extreme points (which we have called the *impelled* and *working*-points) are inversely proportional to the forces which are in equilibrio when applied to these points in the direction of their motion. This is an inductive proposition, and has been used as the foundation of systems of mechanics. It is unnecessary to take up time in proving what is so familiarly known; consequently, the products of the pressures at those points by the velocities of the motions are equal; that is, the product of the pressure actually exerted at the impelled point of a machine working uniformly, multiplied by the velocity of that point, is equal to the product of the resistance actually exerted at the working-point, multiplied by the velocity of that point, that is, by the velocity with which the resistance is overcome,

$$p\,m = r\,n.$$

Now, the product of the resistance, by the velocity with which it is overcome, is evidently the measure of the performance of the machine, or the work done. The product of the actual pressure on the impelled point, by the velocity of that point, may be called the MOMENTUM OF IMPULSE.

101. Hence we deduce this proposition:

In all working machines which have acquired a uniform *motion, the performance of the machine is equal to the momentum of impulse.*[*]

This is a proposition of the utmost importance in the science of machines, and leads to the fundamental maxim of their construction. Since the performance of a machine is equal to the momentum of impulse, it increases and diminishes along with it, and is a maximum when the momentum of impulse is a maximum; therefore, the fundamental maxim in the construction of a machine is to fashion it in such a manner that the momentum of impulse shall be a maximum, or that the product of the pressure actually exerted on the impelled point of the machine by the velocity with which it moves may be as great as possible. Then are we certain that the product of the resistance, by the velocity of the working-point, is as great as possible, provided that we take care that none of the impulse be needlessly wasted by the way by injudicious communications of motion, by friction, by unbalanced loads, and by reciprocal motions, which irrecoverably waste the impelling power.

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[*] The truth of this proposition has been long perceived in every particular instance that happened to engage the attention; but we do not recollect any mechanician before Mr Euler considering it as a general truth, expressing in a few words a mechanical law. This celebrated mathematician undertook, about the year 1735, or 1736, a general and systematic view of machines, in order to found a complete theory immediately conducive to the improvement of practical mechanics In 1743 he published the first propositions of this useful theory in the tenth volume of the *Comment. Petropolitani*, containing the excellent dynamical theorems of which we have given the substance. In the third volume of the *Comment. Novi Petropol.* he prosecuted the subject a little farther; and, in the eighth volume, he entered on what we are now engaged in, and formally announces this fundamental proposition, calling these two products the *momentum of impulse*, and the *momentum of effect*. It is much to be regretted that this consummate mathematician did not continue these useful labours, his ardent mind being carried away by more abstruse speculations in all the most refined departments of mathematics and philosophy. No man in Europe could have prosecuted the subject with more judgment and success.—See also *Mem. Acad. Berlin*, 1747 and 1752.

This maxim holds good whether the resistance remains constantly the same, or varies by any law whatever.

102. But much remains to be done for the improvement of mechanical science before we can avail ourselves of this maxim, and apply it with success. The chief thing, and to this we should give the most unremitting attention, is, to learn the changes which obtain in the actual pressure exerted by those natural powers which we can command; the changes of actual pressure produced by a change of the velocity of the impelled point of the machine. These depend on the specific natures of those powers, and are different in almost every different case. Nothing will more contribute to the improvement of practical mechanics than a series of experiments, well contrived, and accurately made, for discovering those laws of variation, in the cases of those powers which are most frequently employed. Such experiments, however, would be costly, beyond the abilities of an individual; therefore, it were greatly to be wished that public aid were given to some persons of skill in the science to institute a regular train of experiments of this kind. An experimental machine might be constructed, to be wrought either by men or by cattle. This should be loaded with some kind of work which can be very accurately measured, and the load varied at pleasure. When loaded to a certain degree, the men or cattle should be made to work at the rate which they can continue from day to day. The number of turns made in an hour, multiplied by the load, will give the performance corresponding to the velocities; and thus will be discovered the most advantageous rate of motion. The same machine should also be fitted for grinding, for sawing, boring, &c., and similar experiments will discover the relation between the velocities with which these operations are performed, and the resistances which they exert. The laws of friction may be investigated by the same machine. It should also be fitted with a walking-wheel, and the trial should be made of the slope and velocity of walking which gives the greatest momentum of im-

pulse. It is not unreasonable to expect great advantages from such a train of experiments.

103. Till this be done, we must content ourselves with establishing the above, in the most general terms, applicable to any case in which the law of the variation of force may hereafter be discovered.

There is a certain velocity of the impelled point of a machine which puts an end to the action of the moving power. Thus, if the floats of an undershot wheel be moving with the velocity of the stream, no impulse is made on them. If the arm of a gin or capstan be moving with that velocity with which a horse or a man can just move, so as to continue at that speed from day to day, employing all his working strength, but not fatiguing himself; in this state of motion, the animal can exert no pressure on the machine. This may be called the EXTINGUISHING VELOCITY, and we may express it by the symbol e. Let f be that degree of force or pressure which the animal can exert at a dead pull or thrust, as it is called. We do not mean the utmost strain of which the animal is capable, but that which it can continue unremittingly during the working hours of a day, fully employing, but not fatiguing itself. And let p be the pressure which it actually exerts on the impelled point of a machine, moving with the velocity m. Let $e - m$ be called the RELATIVE VELOCITY, and let it be expressed by v. And let it be supposed that it has been discovered, by any means whatever, that the actual pressure varies in the proportion of v^q, or $\overline{e-m}^q$. This supposition gives us $e^q : v^q = f : p$, and $p = f \times \frac{v^q}{e^q}$. For the machine must be at rest, in order that the agent may be able to exert the force f on its impelled point. But when the machine is at rest, what we have named the relative velocity is e, the whole of the extinguishing velocity.

The momentum of impulse is $p\,m$, that is $\frac{v^q}{e^q} f\,m$, or $f \times$

$\frac{v^q}{e^q} \times \overline{e-v}$ (because $m = e - v$.) Therefore $f \times \frac{v^q}{e^q} \times \overline{e-v}$ must be made a maximum. But f and e^q are two quantities which suffer no change. Therefore the momentum of impulse will be a maximum when $v^q \times \overline{e-v}$ is a maximum. Now $v^q \times e - v = v^q e - v^q v, = v^q e - v^{q+1}$. The fluxion of this is $q e v^{q-1} \dot{v} - \overline{q+1}\, v^q \dot{v}$. This being supposed $= 0$, we have the equation

$$q e v^{q-1} = \overline{q+1}\, v^q$$

$$\text{And } q e = \overline{q+1}\, v$$

$$\text{Therefore } v = \frac{q e}{q+1}$$

And m, which is $= e - v$, becomes $\frac{e}{q+1}$. Therefore we must order matters so, that the velocity of the impelled point of the machine may be $= \frac{e}{q+1}$. Now p is $= f \frac{v^q}{e^q}$, and therefore $= f \times \frac{q^q}{\overline{q+1}^q}$. And $p m, = f \frac{p^q}{\overline{q+1}^q} m,$ $= f \frac{q^q}{\overline{q+1}^q} m, = f \frac{p^q}{\overline{q+1}^q} \times \frac{e}{q+1}, = f \times \frac{p^q e}{\overline{q+1}^{q+1}}$ = the momentum of impulse, and therefore = the momentum of effect, or the performance of the machine, when in its best state.

104. Thus may the maxim of construction be said to be brought to a state of great simplicity, and of most easy recollection. A particular case of this maxim has been long known, having been pointed out by Mr Parent. Since the action of bodies depends on their relative velocity, the impulse of fluids must be as the square of the relative velocity. From which Mr Parent deduced, that the most advantageous velocity of the floats of an undershot wheel is one-third of that of the stream. This maxim is evidently included in our general proposition; for, in this case, the index q of that function of the relative velocity v, which is proportional to the impulse, is $= 2$. Therefore we have

the maximum when $v = \frac{2e}{2+1}, = \frac{2}{3}e$, and $m = \frac{1}{3}e$. The extinguishing velocity e, is evidently the velocity of the stream. Our proposition also gives us the precise value of the performance. The impulse of the stream on the float at rest being supposed $= f$, its impulse on the float moving with the velocity $\frac{2}{3}e$ must be $= \frac{4}{9}f$. This is the measure of the actual pressure p. This being multiplied by m, or by $\frac{1}{3}e$, gives $\frac{4}{27}f$. Now f is considered as equal to the weight of a column of water, having the surface of the floatboard for its base, and the depth of the sluice under the surface of the reservoir (or, more accurately, the fall required for generating the velocity of the stream) for its height. Hence it has been concluded, that the utmost performance of an undershot wheel is to raise $\frac{4}{27}$ of the water which impels it, to the height from which it falls. But this is not found very agreeable to observation. Friction, and many imperfections of execution in the delivery of the water, the direction of its impulse, &c. may be expected to make a defalcation from this theoretical performance. But the actual performance, even of mills of acknowledged imperfection, considerably exceeds this, and sometimes is found nearly double of this quantity. The truth is, that the particular fact from which Mr Parent first deduced this maxim, (namely, the performance of what is called *Parent's or Dr Barker's mill,*) is, perhaps, of all that could have been selected, the least calculated for being the foundation of a general rule, being of a nature so abstruse, that the first mathematicians of Europe are to this day doubtful whether they have a just conception of its principles.* Mr Smeaton's experiments shew very distinctly, that the maximum of performance of an undershot wheel corresponds to a velocity considerably greater than one-third of the stream, and approaches nearly to one-half; and he assigns some reasons for this which seem well founded. But, independent of this, the performance of Mr Smeaton's model was much greater than what corresponds with the velocity by the above-mentioned estimation of f. The theory of the impulsion of fluids is extremely imperfect; and Daniel Bernoulli shews, from very unquestionable principles, that the impulse of a narrow vein of fluid on an extended surface is double of what was generally supposed; and his conclusions are abundantly confirmed by the experiments adduced by him.

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* See the Edinburgh Encyclopædia, Art. HYDRODYNAMICS, for a full account of the theory of this curious machine.—ED.

105. It is by no means pretended, that the maxim of construction is reduced to the great simplicity enounced in the proposition now under consideration. We only supposed, that a case had been observed where the pressure exerted by some natural agent did follow the proportions of v^q. This being admitted, the proposition is strictly true. But we do not know any such case; yet is the proposition of considerable use: for we can affirm, on the authority of our own observations, that the action both of men and of draught horses does not deviate very far from the proportions of v^2. The observations were made on men and horses tracking a lighter along a canal, and working several days together, without having any knowledge of the purpose of the observations. The force exerted was first measured by the curvature and weight of the track-rope, and afterwards by a spring steelyard. This was multiplied by the number of yards per hour, and the product considered as the momentum. We found the action of men to be very nearly as $\overline{e-m}^2$. The action of horses, loaded so as not to be able to trot, was nearly as $\overline{e-m}^{1,7}$.

The practitioner can easily avail himself of the maxim, although the function q should never be reduced to any algebraic form. He has only to institute a train of experiments on the natural agent, and select that velocity which gives the highest product when multiplied by its corresponding pressure.

106. When this selection has been made, we have two ways of giving our working-machines the maximum of effect, having once ascertained the pressure f which our natural power exerts on the impelled point of the machine when it is not allowed to move.

1. When the resistance arising from the work, and from friction, is a given quantity; as when water is to be raised to a certain height by a piston of given dimensions.

Since the friction in all the communicating parts of the machine varies in the same proportion with the pressure, and since these vary in the same proportion with the resistance, the sum of the resistance and friction may be represented by $b\,r$, b being an abstract number. Let n be the undetermined velocity of the working-point; or let $m : n$ be the proportion of velocities at the impelled and working-points. Then, because the pressures at these points balance each other, in the case of uniform motion, they are inversely as the velocities at those points. Therefore we must make

$$b r : p = m : n, \text{ and } n = \frac{p m}{b r}, = \frac{\frac{q^q}{\overline{q+1}^q} f m}{b r}, = m \frac{q^q f}{\overline{q+1}^q\, b r},$$

or $m : n = \overline{q+1}^q \times b\,r : q^q f$.

2. On the other hand, when $m : n$ is already given, by the construction of the machine, but $b\,r$ is susceptible of variation, we must load the machine with more and more work, till we have reduced the velocity of its impelled point to $\frac{e}{q+1}$.

In either case, the performance is expressed by what expresses $p\,m$, that is, by $f e \times \frac{q^q}{\overline{q+1}^{q+1}}$. But the useful performance, which is really the work done, will be had by dividing the value now obtained by the number *b*, which expresses the sum of the resistance overcome by the working-point and the friction of the machine.

What has been now delivered contains, we imagine, the chief principles of the theory of machines, and points out the way in which we must proceed in applying them to every case. The reader, we hope, sees clearly the imperfection of a consideration of machines which proceeds no farther than the statement of the proportions of the simultaneous pressures which are excited in all the parts of the machine by the application of the external forces, which we are accustomed to call the *power* and the *weight*. Unless we take also into consideration, the immediate effect of mechanical force applied to body, and combine this with all the pressures which statical principles have enabled us to ascertain, and by this combination be able to say what portion of unbalanced force there is acting at one and all of the pressing points of the machine, and what will be the motion of *every part* of it in consequence of this overplus, we have acquired no knowledge that can be of service to us. We have been contemplating, not a working machine, but a sort of balance. But, by reasoning about these unbalanced forces in the same simple manner as about the fall of heavy bodies, we were able to discover the momentary accelerations of every part, and the sensible motion which it would acquire in any assigned time, if all the circumstances remain the same. We found that the results, although deduced from unquestionable principles, were quite unlike the observed motions of most working machines. Proceeding still on the same principles, we considered this deviation as the indication, and the precise measure, of something which we had not yet attended to, but which the deviation brought into view, and enabled us to ascertain with accuracy. These are the changes which happen in the exertions of our actuating powers by the velocity with which we find it convenient to make them act. Thus we learn more of the nature of those powers; and we found it necessary to distinguish carefully between the apparent magnitude of our actuating power and its real exertion in doing our work. This consideration led us to a fundamental proposition concerning all working machines when they have attained an uniform motion; namely, that the power and resistance then really exerted on the machine precisely balance each other, and that the machine is precisely in the condition of a steelyard loaded with its balanced weights, and moved round its axis by some external force distinct from the power and the weight. We found that this force is the previous overplus of impelling power, before the machine had acquired the uniform motion; and on this occasion we learned to estimate the effect produced, by the momentum (depending on the form of the machine) of the quantity of motion produced in the whole assemblage of power, resistance, and machinery.

107. The theory of machines seemed to be now brought back to that simplicity of equilibrium which we had said was so imperfect a foundation for a theory; but in availing ourselves of the maxim founded on this general proposition, we saw that the equilibrium is of a very different kind from a quiescent equilibrium. It necessarily involves in it the knowledge of the momentary accelerations and their momenta; without which we should not perceive that one state of motion is more advantageous than another, because all give us the same proportion of forces in equilibrio.

But this is not the only use of the previous knowledge of the momentary accelerations of machines; there are many cases where the machine works in this very state. Many machines accelerate throughout while performing their work; and their efficacy depends entirely on the final acceleration. Of this kind is the coining-press, the great forge or tilt-mill, and some other capital engines. The steam-engine, and the common pump, are necessarily of this class, although their efficacy is not estimated by their final acceleration. A great number of engines have reciprocating motions in different subordinate parts. The theory of all such engines requires for its perfection an accurate knowledge of the momentary accelerations; and we must use the formulæ contained in the first part of this article.

108. Still, however, the application of this knowledge has many difficulties, which make a *good* theory of such machines a much more intricate and complicated matter than we have yet led the reader to suppose. In most of these engines, the whole motion may be divided into two parts. One may be called the WORKING-STROKE, and the other, in which the working-points are brought back to a situation which fits them for acting again, may be called the RETURNING-STROKE. This return must be effected either by means of some immediate application of the actuating power, or by some other force, which is counteracted during the working-stroke, and must be considered as making part of the resistance. In the steam-engine, it is generally done by a counterpoise on the outer end of the great working-beam. This must be accounted a part of the resistance, for it must be raised again; and the proportions of the machine for attaining the maximum must be computed accordingly. The quantity of this counterpoise must be adjusted by other considerations. It must be such, that the descent of the pump-rods in the pit may *just employ the whole time* that is necessary for filling the cylinder with steam. If they descend more briskly, (which an unskilful engineer likes to see,) this must be done by means of a greater counterpoise, and this employs more power to raise it again. Desaguliers describes a very excellent machine for raising water in a bucket by a man's stepping into an opposite bucket, and descending by its preponderancy. When he comes to the bottom, he steps out, goes up a stair, and finds the bucket returned and ready to receive him again. This machine is extremely simple, and perhaps

the best that can be contrived; and yet it is one of the most likely to be a very bad one. The bucket into which the man steps must be brought up to its place again by a preponderancy in the machine when unloaded. It may be returned sooner or later. It should arrive precisely at the same time with the man. If sooner, it is of no use, and wastes power in raising a counterpoise which is needlessly heavy; if later, time is lost: therefore, the perfection of this very simple machine requires the judicious combination of two maximums, each of which varies in a ratio compounded of two other ratios. Suppose the man to employ a minute to go up stairs 50 feet, which is very nearly what he can do from day to day as his only work, and suppose him to weigh 150 pounds, and that he acts by means of a simple pulley—the maximum for a lever of equal arms would require him to raise about 60 pounds of water. But when all the other circumstances are calculated, it will be found that he must raise 138 pounds (neglecting the inertia of the machine.) He should raise 542 pounds 10 feet in a minute; and this is nearly the most exact valuation of a man's work.

There is the same necessity of attending to a variety of circumstances in all machines which reciprocate in the whole or any considerable part of their motion. The force employed for bringing the machine into another working position, must be regulated by the time necessary for obtaining a new supply of power; and then the proportion of *m* to *n* must be so adjusted, that the work performed, divided by the *whole* time of the working and returning strokes, may give the greatest quotient. It is still a difficult thing, therefore, to construct a machine in the most perfect manner, or even to say what will be the performance of a machine already constructed; yet we see that every circumstance is susceptible of accurate computation.

With respect to machines which acquire a sort of uniform motion in general, although subject to partial reciprocations, as in a pumping, stamping, forging-engine, it is also difficult to assign the rate even of this general uniform motion. We may, however, say, that it will not be greater than if it were uniform throughout. Were it entirely free from friction, it would be exactly the same as if uniform; because the accelerations during the advantageous situations of the impelling power would compensate the retardations. But friction diminishes the accelerations, without diminishing the retardations.

We may conclude this article with some observations tending to the general improvement of machines.

109. Nothing contributes more to the perfection of a machine, especially such as is massive and ponderous, than great uniformity of motion. Every irregularity of motion wastes some of the impelling power; and it is only the greatest of the varying velocities which is equal to that which the machine would acquire if moving uniformly throughout; for, while the motion accelerates, the impelling force is greater than what balances the resistance then actually opposed to it, and the velocity is less than what the machine would acquire if moving uniformly; and when the machine attains its greatest velocity, it attains it because the power is then not acting against the whole resistance. In both of these situations, therefore, the performance of the machine is less than if the power and resistance were exactly balanced; in which case it would move uniformly.

110. Every attention should, therefore, be given to this, and we should endeavour to remove all cause of irregularity. The communications of motion should be so contrived, that if the impelled point be moving uniformly, by the uniform pressure of the power, the working-point shall also be moving uniformly. Then we may generally be certain, that the massy parts of the machine will be moving uniformly. When this is not done through the whole machine, there are continual returns of strains and jolts; the inertia of the different parts acting in opposite directions. Although the

whole momenta may always balance each other, yet the general motion is hobbling, and the points of support are strained. A great engine so constructed, commonly causes the building to tremble; but when uniform motion pervades the whole machine, the inertia of each part tends to preserve this uniformity, and all goes smoothly. It is also deserving of remark, that when the communications are so contrived that the uniform motion of one part produces uniform motion on the next, the pressures at the communicating points remain constant or invariable. Now the accomplishing of this is always within the reach of mechanics.

111. One of the most usual communications in machinery is by means of toothed wheels acting on each other. It is of importance to have the teeth so formed, that the pressure by which one of them A urges the other B round its axis, shall be constantly the same. It can easily be demonstrated, that when this is the case, the uniform angular motion of the one will produce a uniform angular motion of the other; or, if the motions are thus uniform, the pressures are invariable. This is accomplished on this principle, that the mutual actions of solid bodies on each other in the way of pressure, are perpendicular to the touching surfaces. Therefore, let the tooth *a* (Plate VIII. fig. 1.) press on the tooth *b* in the point C; and draw the line FCDE perpendicular to the touching surfaces in the point C. Draw AF, BE perpendicular to FE, and let FE cut the line AB in D. It is plain, from the common principles of mechanics, that if the line FE, drawn in the manner now described, always pass through the same point D, whatever may be the situation of the acting teeth, the mutual action of the wheels will always be the same. It will be the same as if the arm AD acted on the arm BD. In the treatises on the construction of mills, and other works of this kind, are many instructions for the formation of the teeth of wheels; and almost every noted millwright has his own nostrums. Most of them are egregiously faulty in respect of mechanical principle. In-

deed, they are little else than instructions how to make the teeth clear each other without sticking. Mr de la Hire first pointed out the above-mentioned principle, and justly condemned the common practice of making the small wheel or pinion in the form of a lantern, (whence it also took its name) consisting of two round disks, having a number of cylindrical spokes (fig. 2.) The slightest inspection of this construction shews, that, in the different situations of the working-teeth, the line FCE continually changes its intersection with AB. If the wheel B be very small in comparison of the other, and if the teeth of A take deep hold of the cylindrical pins of B, the line of action EF is sometimes so disadvantageously placed, that the pressure of the one wheel has scarcely any tendency at all to turn the other. Mr de la Hire, or Dr Hooke,* was, we think, the first who investigated the form of tooth which procured this constant action between the wheels; and, in a very ingenious dissertation, published among the Memoirs of the Academy of Sciences at Paris, 1668, the former of these gentlemen shews, that this will be ensured by forming the teeth into epicycloids. Mr Camus, of the same academy, has published an elaborate dissertation on the same subject, in which he prosecutes the principle of Mr de la Hire, and applies it to all the variety of cases which can occur in practice.† There is no doubt as to the goodness of the principle; and it has another excellent property, "that the mutual action of the teeth is absolutely without any friction." The one tooth only applies itself to the other, and rolls on it, but does not slide or rub in the smallest degree. This makes them last long, or rather does not allow them to wear in the least.

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* This property of the Epicycloid was first pointed out by the celebrated Danish astronomer, Olaus Roemer.

† Camus's Dissertation on the Teeth of Wheels, as published in his *Cours de Mathematique*, has been translated into English, and was published in 1806.

But the construction is subject to a limitation which must not be neglected. The teeth must be so made, that the curved part of the tooth *b* is acted on by a flat part of the tooth *a* till it comes to the line AB in the course of its action; after which, the curved part of *a* acts on a flat part of *b*; or the whole action of *a* on *b* is either completed, or only begins at the line AB, joining the centres of the wheels.

112. Another form of the teeth secures the perfect uniformity of action without this limitation, which requires very nice execution. Let the teeth of each wheel be formed by evolving its circumference; that is, let the acting face GCH of the tooth *a* have the form of the curve traced by the extremity of the thread FC, unlapped from the circumference. In like manner, let the acting face of the tooth *b* be formed by unlapping a thread from its circumference. It is evident, that the line FCE, which is drawn perpendicularly to the touching surfaces in the point C, is just the direction or position of the evolving threads by which the two acting faces are formed. This line must therefore be the common tangent to the two circles or circumferences of the wheels, and will therefore always cut the line AB in the same point D. This form allows the teeth to act on each other through the whole extent of the line FCE, and therefore will admit of several teeth to be acting at the same time (twice the number that can be admitted in Mr de la Hire's method.) This, by dividing the pressure among several teeth, diminishes its quantity on any one of them, and therefore diminishes the dents or impressions which they unavoidably make on each other. It is not altogether free from sliding and friction, but the whole of it can hardly be said to be sensible. The whole slide of a tooth three inches long, belonging to a wheel of ten feet diameter, acting on a tooth of a wheel of two feet diameter, does not amount to $\frac{1}{60}$th of an inch, a quantity altogether insignificant.

In the formation of the teeth of wheels, a small deviation from these perfect forms is not perhaps of very great importance, except in cases where a very large wheel drives a very small one (a thing which a good engineer will always avoid.) As the construction, however, is exceedingly easy, it would be unpardonable to omit it. Well-formed teeth, and a great number of them acting at once, make the communication of motion extremely smooth and uniform. The machine works without noise, and the teeth last a very long time without sensibly changing their shape. But there are cases, such as the pallets of clocks and watches, where the utmost accuracy of form is of the greatest importance for the perfection of the work.

113. When heavy stampers are to be raised, in order to drop on the matters to be pounded, the wipers by which they are lifted should be made of such a form that the stamper may be raised by a uniform pressure, or with a motion almost perfectly uniform. If this is not attended to, and the wiper is only a pin sticking out from the axis, the stamper is forced into motion at once. This occasions violent jolts to the machine, and great strains on its moving parts and their points of support; whereas, when they are gradually lifted, the inequality of desultory motion is never felt at the impelled point of the machine. We have seen pistons moved by means of a double rack on the piston-rod. A half wheel takes hold of one rack, and raises it to the required height. The moment the half wheel has quitted that side of the rack, it lays hold of the other side, and forces the piston down again. This is proposed as a great improvement; correcting the unequable motion of the piston moved in the common way by a crank. But it is far inferior to the crank motion. It occasions such abrupt changes of motion, that the machine is shaken by jolts. Indeed, if the movement were accurately executed, the machine would be shaken to pieces, if the parts did not give way by bending and yielding. Accordingly, we have always observed that this motion soon failed, and was changed for one that was more smooth. A judicious engineer

will avoid all such sudden changes of motion, especially in any ponderous part of a machine.*

When several stampers, pistons, or other reciprocal movers, are to be raised and depressed, common sense teaches us to distribute their times of action in a uniform manner, so that the machine may always be equally loaded with work. When this is done, and the observations in the preceding paragraph attended to, the machine may be made to move almost as smoothly as if there were no reciprocations in it. Nothing shews the ingenuity of the author more than the artful, yet simple and effectual contrivances, for obviating those difficulties that unavoidably arise from the very nature of the work that must be performed by the machine, and of the power employed. The inventive genius and sound judgment of Watt and Boulton are as perceptible to a skilled observer, in these subordinate parts of some of their great engines, as in the original discovery on which their patent is founded. In some of those engines, the mass of dead matter which must be put into motion, and this motion destroyed and again restored in every stroke, is enormous, amounting to above an hundred tons. The ingenious authors have even contrived to draw some advantages from it, by allowing a great want of equilibrium in certain positions; and this has been condemned as a blunder by engineers who did not see the use made of it.

114. There is also great room for ingenuity and good choice in the management of the moving power, when it is such as cannot immediately produce the kind of motion required for effecting the purpose. We mentioned the conversion of the continued rotation of an axis into the reciprocating motion of a piston, and the improvement which was

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* For farther information on this subject, see Camus' *Cours de Mathematique*, § 550; Robertson Buchanan's *Essay on the Teeth of Wheels*, London. 1808; and Ferguson's Lectures, vol. ii. Appendix.—Ed.

thought to have been made on the common and obvious contrivance of a crank, by substituting a double rack on the piston-rod, and the inconvenience arising from the jolts occasioned by this change. We have seen a great forge, where the engineer, in order to avoid the same inconvenience arising from the abrupt motion given to the great sledge-hammer of seven hundred weight, resisting with a five-fold momentum, formed the wipers into spirals, which communicated motion to the hammer almost without any jolt whatever; but the result was, that the hammer rose no higher than it had been raised in contact with the wipers, and then fell on the iron bloom with very little effect. The cause of its inefficiency was not guessed at; but it was removed, and wipers of the common form were put in place of the spirals. In this operation, the rapid motion of the hammer is absolutely necessary. It is not enough to *lift* it up; it must be *tossed* up, so as to fly higher than the wiper lifts it, and to strike with great force the strong oaken spring which is placed in its way. It compresses this spring, and is reflected by it with a considerable velocity, so as to hit the iron as if it had fallen from a great height. Had it been allowed to fly to that height, it would have fallen upon the iron with somewhat more force (because no oaken spring is perfectly elastic,) but this would have required more than twice the time.

115. In employing a power which of necessity reciprocates, to drive machinery which requires a continuous motion, (as in applying the steam-engine to a cotton or a grist-mill,) there also occur great difficulties. The necessity of reciprocation in the first mover wastes much power; because the instrument which communicates such an enormous force must be extremely strong, and be well supported. The impelling power is wasted in imparting, and afterwards detroying, a vast quantity of motion in the working beam. The skilful engineer will attend to this, and do his utmost to procure the necessary strength of this

first mover, without making it a vast load of inert matter. He will also remark, that all the strains on it, and on its supports, are changing their directions in every stroke. This requires particular attention to the manner of supporting it. If we observe the steam-engines which have been long erected, we see that they have uniformly shaken the building to pieces. This has been owing to the ignorance or inattention of the engineer in this particular. They are much more judiciously erected now, experience having taught the most ignorant that no building can withstand their desultory and opposite jolts, and that the great movements must be supported by a frame-work independent of the building of masonry which contains it.*

The engineer will also remark, that when a single-stroke steam-engine is made to turn a mill, all the communications of motion change the direction of their pressure twice every stroke. During the working-stroke of the beam, one side of the teeth of the intervening wheels is pressing the machinery forward; but during the returning stroke, the machinery, already in motion, is dragging the beam, and the wheels are acting with the other side of the teeth. This occasions a rattling at every change, and makes it proper to fashion both sides of the teeth with the same care.

It will frequently conduce to the good performance of an engine, to make the action of the resisting work unequable, accommodated to the inequalities of the impelling power. This will produce a more uniform motion in machines in which the momentum of inertia is inconsiderable. There are some beautiful specimens of this kind of adjustment in the mechanism of animal bodies.

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* The gudgeons of a water-wheel should never rest on the wall of the building. It shakes it; and if set up soon after the building has been erected, it prevents the mortar from taking firm bond; perhaps by shattering the calcareous crystals as they form. When the engineer is obliged to rest the gudgeons in this way, they should be supported by a block of oak laid a little hollow. This softens all tremors, like the springs of a wheel carriage. This practice would be very serviceable in many other parts of the construction.

116. It is very customary to add what is called a FLY to machines. This is a heavy disk or hoop, or other mass of matter, *balanced on its axis*, and so connected with the machinery as to turn briskly round with it. This may be done with the view of rendering the motion of the whole more regular, notwithstanding unavoidable inequalities of the accelerating forces, or of the resistances occasioned by the work. It becomes a REGULATOR. Suppose the resistance extremely unequal, and the impelling power perfectly constant; as when a bucket wheel is employed to work *one* pump. When the piston has ended its working-stroke, and while it is going down the barrel, the power of the wheel being scarcely opposed, it accelerates the whole machine, and the piston arrives at the bottom of the barrel with a considerable velocity. But in the rising again, the wheel is opposed by the column of water now pressing on the piston. This immediately retards the wheel; and when the piston has reached the top of the barrel, all the acceleration is undone, and is to begin again. The motion of such a machine is very hobbling; but the surplus of accelerating force at the beginning of a returning-stroke will not make such a change in the motion of the machine if we connect the fly with it. For the accelerating momentum is a determinate quantity. Therefore, if the radius of the fly be great, this momentum will be attained by communicating a small angular motion to the machine. The momentum of the fly is as the square of its radius; therefore it resists acceleration in this proportion; and although the overplus of power generates the same momentum of rotation in the whole machine as before, it makes but a small addition to its velocity. If the diameter of the fly be doubled, the augmentation of rotation will be reduced to one-fourth. Thus, by giving a rapid motion to a small quan-

tity of matter, the great acceleration during the returning-stroke of the piston is prevented. This acceleration continues, however, during the whole of the returning-stroke, and at the end of it the machine has acquired its greatest velocity. Now the working-stroke begins, and the overplus of power is at an end. The machine accelerates no more; but if the power is just in equilibrio with the resistance, it keeps the velocity which it has acquired, and is still more accelerated during the *next* returning-stroke. But now, at the beginning of the subsequent working-stroke, there is an overplus of resistance, and a retardation begins, and continues during the whole rise of the piston; but it is inconsiderable in comparison of what it would have been without the fly; for the fly, retaining its acquired momentum, drags forward the rest of the machine, aiding the impelling power of the wheel. It does this by all the communications taking into each other in the opposite direction. The teeth of the intervening wheels are heard to drop from their former contact on one side, to a contact on the other. By considering this process with attention, we easily perceive that, in a few strokes, the overplus of power during the returning-stroke comes to be so adjusted to the deficiency during the working-stroke, that the accelerations and retardations exactly destroy each other, and every succeeding stroke is made with the same velocity, and an equal number of strokes is made in every succeeding minute. Thus the machine acquires a general uniformity with periodical inequalities. It is plain, that by sufficiently enlarging either the diameter or the weight of the fly, the irregularity of the motion may be rendered as small as we please. It is much better to enlarge the diameter. This preserves the friction more moderate, and the pivot wears less. For these reasons, a fly is in general a considerable improvement in machinery, by equalizing many exertions that are naturally very irregular. Thus, a man working at a common windlass, exerts a very irregular pressure on the winch. In one of his positions in each turn he can exert a force of near 70 pounds without fatigue, but in another he cannot exert above 25; nor must he be loaded with much above this in general. But if a large fly be connected properly with the windlass, he will act with equal ease and speed against 30 pounds.

117. This regulating power of the fly is without bounds, and may be used to render uniform a motion produced by the most desultory and irregular power. It is thus that the most regular motion is given to mills that are driven by a single-stroke steam-engine, where for two or even three seconds there is no force pressing round the mill. The communication is made through a massive fly of very great diameter, whirling with great rapidity. As soon as the impulse ceases, the fly, continuing its motion, urges round the whole machinery with almost unabated speed. At this instant all the teeth, and all the joints, between the fly and the first mover, are heard to catch in the opposite direction.

If any permanent change should happen in the impelling power, or in the resistance, the fly makes no obstacle to its producing its full effect on the machine; and it will be observed to accelerate or retard uniformly, till a new general speed is acquired exactly corresponding with this new power and resistance.

Many machines include in their construction movements which are equivalent with this intentional regulator. A flour-mill, for example, cannot be better regulated than by its millstone; but, in the Albion mills, a heavy fly was added with great propriety; for if the mills had been regulated by their millstones only, then at every change of stroke in the steam-engine, the whole train of communications between the beam, which is the first mover, and the regulating millstone, which is the very last mover, would take in the opposite direction. Although each drop in the teeth and joints be but a trifle, the whole, added together, would make a considerable jolt. This is avoided by a regulator immediately adjoining to the beam. This continually presses the working-machinery in one direction. So judiciously were the movements of that noble machine contrived, and so nicely were they executed, that not the least noise was heard, nor the slightest tremor felt in the building.

Mr Valoué's beautiful pile-engine, employed at Westminster Bridge, is another remarkable instance of the regulating powers of a fly. When the ram is dropped, and its follower disengaged immediately after it, the horses would instantly tumble down, because the load, against which they had been straining hard, is at once taken off; but the gin is connected with a very large fly, which checks any remarkable acceleration, allowing the horses to lean on it during the descent of the load; after which their draught recommences immediately. The spindles, cards, and bobbins, of a cotton-mill, are also a sort of flies. Indeed all bulky machines of the rotative kind tend to preserve their motion with some degree of steadiness, and their great momentum of inertia is as useful in this respect as it is prejudicial to the acceleration or any reciprocation when wanted.

118. There is another kind of regulating-fly, consisting of wings whirled briskly round till the resistance of the air prevents any great acceleration. This is a very bad one for a *working* machine, for it produces its effect by *really wasting* a part of the moving power. Frequently it employs a very great and unknown part of it, and robs the proprietor of much work. It should never be introduced into any machine employed in manufactures.

119. Some rare cases occur where a very different regulator is required; where a certain determined velocity is found necessary. In this case the machine is furnished, at its extreme mover, with a conical pendulum, consisting of two heavy balls hanging by rods, which move in very

nice and steady joints at the top of a vertical axis. It is well known, that when this axis turns round, with an angular velocity suited to the length of those pendulums, the time of a revolution is determined. Thus, if the length of each pendulum be 39⅛ inches, the axis will make a revolution in two seconds very nearly. If we attempt to force it more swiftly round, the balls will recede a little from the axis, but it employs as long time for a revolution as before; and we cannot make it turn swifter, unless the impelling power be increased beyond all probability; in which case the pendulum will fly out from the centre till the rods are horizontal, after which every increase of power will accelerate the machine very sensibly. Watt and Boulton have applied this contrivance with great ingenuity to their steam-engines, when they are employed for driving machinery for manufactures which have a very changeable resistance, and where a certain speed cannot be much departed from without great inconvenience. They have connected this recess of the balls from the axis (which gives immediate indication of an increase of power or a diminution of resistance) with the cock which admits the steam to the working-cylinder. The balls flying out, cause the cock to close a little, and diminish the supply of steam. The impelling power diminishes the next moment, and the balls again approach the axis, and the rotation goes on as before, although there may have occurred a very great excess or deficiency of power. The same contrivance may be employed to raise or lower the feeding sluice of a water-mill employed to drive machinery.

120. A fly is sometimes employed for a different purpose from that of a regulator of motion: It is employed as a *collector of power*. Suppose all resistance removed from the working-point of a machine furnished with a very large or heavy fly immediately connected with the working-point. When a small force is applied to the impelled point of this machine, motion will begin in the machine, and the fly

122. EAB (fig. 3.) is the barrel of the capstan, standing vertically in a proper frame, as usual, and urged round by bars such as EF. The upper part A of the barrel is 17 inches in diameter, and the lower B is 16. C is a strong pulley 16 inches in diameter, having a hook D, which takes hold of a hawser attached to the load. The rope ACB is wound round the barrel A, passes over the pulley C, and is then wound round the barrel B in the opposite direction. No farther description is necessary, we think, to shew that, by heaving by the bar F, so as to wind more of the rope upon A, and unwind it from B, the pulley C must be brought nearer to the capstan by about three inches for each turn of the capstan; and that this simple capstan is equivalent to an ordinary capstan of the same length of bar EF, and diameter of barrel B, combined with a sixteen-fold tackle of pulleys; or, in short, that it is 16 times more powerful than the common capstan, free from the great loss by friction and bending of ropes, which would absorb a third of the power of a sixteen-fold tackle; and that whereas all other engines become weaker as they multiply the power to a greater degree, (unless they are proportionally more bulky) this engine becomes really stronger in itself. Suppose we wanted to have it twice as powerful as at present, nothing is necessary but to cover the part B of the barrel with laths a quarter of an inch thick. In short, the nearer the two barrels are to equality, the more powerful does it become. We give it to the public as an excellent capstan, and as suggesting thoughts which an intelligent engineer may employ with great effect. By this contrivance, and using an iron wire instead of a catgut, we converted a common eight-day clock into one which goes for two months.

We have now established the principles on which machines must be constructed, in order that they may produce the greatest effect; but it would be improper to dismiss the subject without stating to our readers Mr Bramah's new method of producing and applying a more considerable degree of power to all kinds of machinery requiring motion and force, than by any means at present practised for that purpose. This method, for which, on the 31st of March, 1796, he obtained a patent, consists in the application of water, or other dense fluids, to various engines, so as, in some instances, to cause them to act with immense force; in others, to communicate the motion and powers of one part of a machine to some other part of the same machine; and, lastly, to communicate the motion and force of one machine to another, where their local situations preclude the application of all other methods of connection.

The first and most material part of this invention will be clearly understood by an inspection of fig. 4., where "A is a cylinder of iron, or other materials, sufficiently strong, and bored perfectly smooth and cylindrical; into which is fitted the piston B, which must be made perfectly water-tight, by leather or other materials, as used in pump-making. The bottom of the cylinder must also be made sufficiently strong with the other part of the surface, to be capable of resisting the greatest force or strain that may at any time be required. In the bottom of the cylinder is inserted the end of the tube C, the aperture of which communicates with the inside of the cylinder, under the piston B, where it is shut with the small valve D, the same as the suction-pipe of a common pump. The other end of the tube C communicates with the small forcing-pump or injector E, by means of which, water, or other dense fluids, can be forced or injected into the cylinder A, under the piston B. Now, suppose the diameter of the cylinder A to be 12 inches, and the diameter of the piston of small pump or injector E only one quarter of an inch, the proportion between the two surfaces or ends of the said pistons will be as 1 to 2304; and supposing the intermediate space between them to be filled with water, or other dense fluid, capable of sufficient resistance, the force of one piston will act on the other just in the above proportion, viz. as 1 is to 2304. Suppose the small piston in the injector to be forced down when in the act of pumping or injecting water into the cylinder A, with the power of 20 cwt., which could easily be done by the lever H; the piston B would then be moved up with a force equal to 20 cwt. multiplied by 2304. Thus is constructed a hydro-mechanical engine, whereby a weight amounting to 2304 tons can be raised by a simple lever, through equal space, in much less time than could be done by any apparatus constructed on the known principles of mechanics; and it may be proper to observe, that the effect of all other mechanical combinations is counteracted by an accumulated complication of parts, which renders them incapable of being usefully extended beyond a certain degree; but in machines acted upon or constructed on this principle, every difficulty of this kind is obviated, and their power subject to no finite restraint. To prove this, it will be only necessary to remark, that the force of any machine acting upon this principle can be increased *ad infinitum*, either by extending the proportion between the diameter of the injector and the cylinder A, or by applying greater power to the lever H.

" Fig. 5. represents the section of an engine, by which very wonderful effects may be produced instantaneously by means of compressed air. AA is a cylinder, with the piston B fitting air-tight, in the same manner as described in fig. 4. C is a globular vessel made of copper, iron, or other strong materials, capable of resisting immense force, similar to those of air-guns. D is a strong tube of small bore, in which is the stop-cock E. One of the ends of this tube communicates with the cylinder under the piston B, and the other with the globe C. Now, suppose the cylinder A to be the same diameter as that in fig. 4., and the tube D equal to one-quarter of an inch diameter, which is the same as the injector, fig. 4.: then, suppose that air it injected into the globe C (by the common method) till it presses against the cock E with a force equal to 20 cwt., which can easily be done; the consequence will be, that when the cock E is opened, the piston B will be moved in the cylinder AA with a power or force equal to 2304 tons; and it is obvious, as in the case fig. 4., that any other unlimited degree of force may be acquired by machines or engines thus constructed.

" Fig. 6. is a section merely to show how the power and motion of one machine may, by means of fluids, be transferred or communicated to another, let their distance and local situation be what they may. A and B are two small cylinders, smooth and cylindrical; in the inside of each of which is a piston, made water and air-tight, as in figs. 4. and 5. CC is a tube conveyed under ground, or otherwise, from the bottom of one cylinder to the other, to form a communication between them, notwithstanding their distance be ever so great; this tube being filled with water, or other fluid, until it touch the bottom of each piston; then, by depressing the piston A, the piston B will be raised. The same effect will be produced *vice versa:* thus bells may be rung, wheels turned, or other machinery put invisibly in motion, by a power being applied to either.

" Fig. 7. is a section, shewing another instance of communicating the action and force of one machine to another; and how water may be raised out of wells of any depth, and at any distance from the place where the operating power is applied. A is a cylinder of any required dimensions, in which is the working-piston B, as in the foregoing examples; into the bottom of this cylinder is inserted the tube C, which may be of less bore than the cylinder A. This tube is continued, in any required direction, down to the pump cylinder D, supposed to be fixed in the deep well EE, and forms a junction therewith above the piston F; which piston has a rod G, working through the stuffing-box, as is usual in a common pump. To this rod G is connected, over a pulley or otherwise, a weight H sufficient to overbalance the weight of the water in the tube C, and to raise the piston F when the piston B is lifted: thus, suppose the piston B is drawn up by its rod, there will be a vacuum made in the pump cylinder D, below the piston F; this vacuum will be filled with water through the suction-pipe, by the pressure of the atmosphere, as in all pumps fixed in air. The return of the piston B, by being pressed downwards in the cylinder A, will make a stroke of the piston in the pump cylinder D, which may be repeated in the usual way by the motion of the piston B, and the action of the water in the tube C. The rod G of the piston F, and the weight H, are not necessary in wells of a depth where the atmosphere will overbalance the water in the suction of the pump cylinder D, and that in the tube C. The small tube and cock in the cistern I, are for the purpose of charging the tube C."*

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* A full account of Bramah's hydrostatic crane, with correct drawings, taken from Mr Bramah's own machine, will be found in the Edinburgh Encyclopædia, Article CRANE.—ED.

RESISTANCE OF FLUIDS.

Of all the resistances of bodies to each other, there is undoubtedly none of greater importance than the resistance or re-action of fluids. It is here that we must look for a theory of naval architecture, for the impulse of the air is our moving power, and this must be modified so as to produce every motion we want by the form and disposition of our sails; and it is the resistance of the water which must be overcome, that the ship may proceed in her course; and this must also be modified to our purpose, that the ship may not drive like a log to leeward, but, on the contrary, may ply to windward, that she may answer her helm briskly, and that she may be easy in all her motions on the surface of the troubled ocean. The impulse of wind and water makes them ready and indefatigable servants in a thousand shapes for driving our machines; and we should lose much of their service did we remain ignorant of the laws of their action: they would sometimes become terrible masters, if we did not fall upon methods of eluding or softening their attacks.

We cannot refuse the ancients a considerable knowledge of this subject. It was equally interesting to them as to us; and we cannot read the accounts of the naval exertions of Phœnicia, Carthage, and of Rome, exertions which have not been surpassed by any thing of modern date, without believing that they possessed much practical and experimental knowledge of this subject. It was not, perhaps, possessed by them in a strict and systematic form, as it is now taught by our mathematicians; but the master-builders, in their dock-yards, did undoubtedly exercise their genius in comparing the forms of their finest ships, and in marking those circumstances of form and dimension which were *in fact* accompanied with the desirable properties of a ship, and thus framing to themselves maxims of naval architecture in the same manner as we do now. For we believe that our naval architects are not disposed to grant that they have profited much by all the labours of the mathematicians. But the ancients had not made any great progress in the physicomathematical sciences, which consist chiefly in the application of calculus to the phenomena of nature. In this branch they could make none, because they had not the means of investigation. A knowledge of the motions and actions of fluids is accessible only to those who are familiarly acquainted with the fluxionary mathematics; and without this key there is no admittance. Even when possessed of this guide, our progress has been very slow, hesitating, and devious; and we have not yet been able to establish any set of doctrines which are susceptible of an easy and confident application to the arts of life. If we have advanced farther than the ancients, it is because we have come after them, and have profited by their labours, and even by their mistakes.

Sir Isaac Newton was the first (as far as we can recollect) who attempted to make the motions and actions of fluids the subject of mathematical discussion. He had invented the method of fluxions long before he engaged in his physical researches; and he proceeded in these *suâ mathesi facem præferente.* Yet even with this guide he was often obliged to grope his way, and to try various bye-paths, in the hopes of obtaining a legitimate theory. Having exerted all his powers in establishing a theory of the lunar motions, he was obliged to rest contented with an approximation instead of a perfect solution of the problem which ascertains the motions of three bodies mutually acting on each other. This convinced him that it was in vain to expect an accurate investigation of the motions and actions of fluids, where millions of unseen particles combine their influence. He therefore cast about to find some particular case of the problem which would admit of an accurate determination, and at the same time furnish circumstances of analogy or resemblance sufficiently numerous for giving limiting cases, which should include between them those other cases that did not admit of this accurate investigation. And thus, by knowing the limit to which the case proposed did approximate, and the circumstance which regulated the approximation, many useful propositions might be deduced for directing us in the application of these doctrines to the arts of life.

He therefore figured to himself a hypothetical collection of matter which possessed the characteristic property of fluidity, viz. the *quâquâversus* propagation of pressure, and the most perfect intermobility (pardon the uncouth term) of parts, and which formed a physical whole or aggregate, whose parts were connected by mechanical forces, determined both in degree and in direction, and such as rendered the determination of certain important circumstances of their motion susceptible of precise investigation. And he concluded, that the laws which he should discover in these motions must have a great analogy with the laws of the motions of real fluids: And from this hypothesis he deduced a series of propositions, which form the basis of almost all the theories of the impulse and resistance of fluids which have been offered to the public since his time.

It must be acknowledged, that the results of this theory agree but ill with experiment, and that, *in the way in which it has been zealously prosecuted by subsequent mathematicians*, it proceeds on principles or assumptions which are not only gratuitous, but even false. But it affords such a beautiful application of geometry and calculus, that mathematicians have been as it were fascinated by it, and have published systems so elegant and so extensively applicable, that one cannot help lamenting that the foundation is so flimsy. John Bernoulli's theory, in his dissertation on the communication of motion, and Bouguer's, in his *Traité du Navire*, and in his *Theorie du Manœuvre et de la Mâture des Vaisseaux*, must ever be considered as among the finest specimens of physicomathematical science which the world has seen. And, with all its imperfections, this theory still furnishes (as was expected by its illustrious author) many propositions of immense practical use, they being the limits to which the real phenomena of the impulse and resistance of fluids really approximate. So that when the law by which the phenomena deviate from the theory is once determined by a well-chosen series of experiments, this hypothetical theory becomes almost as valuable as a true one. And we may add, that although Mr d'Alembert, by treading warily in the steps of Sir Isaac Newton in another route, has discovered a genuine and unexceptionable theory, the process of investigation is so intricate, requiring every finesse of the most abstruse analysis, and the final equations are so complicated, that even their most expert author has not been able to deduce more than one simple proposition (which too was discovered by Daniel Bernoulli by a more simple process) which can be applied to any use. The hypothetical theory of Newton, therefore, continues to be the ground-work of all our practical knowledge of the subject.

We shall therefore lay before our readers a very short view of the theory, and the manner of applying it. We shall then show its defects (all of which were pointed out by its great author), and give a historical account of the many attempts which have been made to amend it or to substitute another: in all which we think it our duty to show, that Sir Isaac Newton took the lead, and pointed out every path which others have taken, if we except Daniel Bernoulli and d'Alembert; and we shall give an account of the chief sets of experiments which have been made on this important subject, in the hopes of establishing an empirical theory, which may be employed with confidence in the arts of life.

We know by experience that force must be applied to a body in order that it may move through a fluid, such as air or water; and that a body projected with any velocity is gradually retarded in its motion, and generally brought to rest. The analogy of nature makes us imagine that there is a force acting in the opposite direction, or opposing the motion, and that this force resides in, or is exerted by, the fluid. And the phenomena resemble those which accompany the known resistance of active beings, such as animals. Therefore we give to this supposed force the metaphorical name of RESISTANCE. We also know that a fluid in motion will hurry a solid body along with the stream, and that it requires force to maintain it in its place. A similar analogy makes us suppose that the fluid exerts force, in the same manner as when an active being impels the body before him; therefore we call this the *Impulsion of a Fluid.* And as our knowledge of nature informs us that the mutual actions of bodies are in every case equal and opposite, and that the observed change of motion is the only indication, characteristic, and measure, of the changing force, the forces are the same (whether we call them impulsions or resistances) when the relative motions are the same, and therefore depend entirely on these relative motions. The force, therefore, which is necessary for keeping a body immoveable in a stream of water, flowing with a certain velocity, is the same with what is required for moving this body with this velocity through stagnant water. To any one who admits the motion of the earth round the sun, it is evident that we can neither observe nor reason from a case of a body moving through still water, nor of a stream of water pressing upon or impelling a quiescent body.

A body in motion appears to be resisted by a stagnant fluid, because it is a law of mechanical nature that force must be employed in order to put any body in motion. Now the body cannot move forward without putting the contiguous fluid in motion, and force must be employed for producing this motion. In like manner, a quiescent body is impelled by a stream of fluid, because the motion of the contiguous fluid is diminished by this solid obstacle; the resistance, therefore, or impulse, no way differs from the ordinary communications of motion among solid bodies.

Sir Isaac Newton, therefore, begins his theory of the resistance and impulse of fluids, by selecting a case where, although he cannot pretend to ascertain the motions themselves which are produced in the particles of a contiguous fluid, he can tell precisely their mutual ratios.

He supposes two systems of bodies such, that each body of the first is similar to a corresponding body of the second, and that each is to each in a constant ratio. He also supposes them to be similarly situated, that is, at the angles of similar figures, and that the homologous lines of these figures are in the same ratio with the diameters of the bodies. He farther supposes, that they attract or repel each other in similar directions, and that the accelerating connecting forces are also proportional; that is, the forces in the one system are to the corresponding forces in the other system in a constant ratio, and that, in each system taken apart, the forces are as the squares of the velocities directly, and as the diameters of the corresponding bodies, or their distances, inversely.

This being the case, it legitimately follows, that if similar parts of the two systems are put into similar motions, in any given instant, they will *continue* to move similarly, each correspondent body describing similar curves, with proportional velocities: For the bodies being similarly situated, the forces which act on a body in one system, arising from the combination of any number of adjoining particles, will have the same direction with the force acting on the corresponding body in the other system, arising from the combined action of the similar and similarly directed forces of the adjoining corresponding bodies of the other system; and these compound forces will have the same ratio with the simple forces which constitute them, and will be as the squares of the velocities directly, and as the distances, or any homologous lines inversely; and therefore the chords of curvature, having the direction of the centripetal or centrifugal forces, and similarly inclined to the tangents of the curves described by the corresponding bodies, will have the same ratio with the distances of the particles. The curves described by the corresponding bodies will therefore be similar, the velocities will be proportional, and the bodies will be similarly situated at the end of the first moment, and exposed to the action of similar and similarly situated centripetal or centrifugal forces; and this will again produce similar motions during the next moment, and so on for ever. All this is evident to any person acquainted with the elementary doctrines of curvilineal motions, as delivered in the theory of physical astronomy.

From this fundamental proposition, it clearly follows, that if two similar bodies, having their homologous lines proportional to those of the two systems, be similarly projected among the bodies of those two systems with any velocities, they will produce similar motions in the two systems, and will themselves continue to move similarly; and therefore will, in every subsequent moment, suffer similar diminutions or retardations. If the initial velocities of projection be the same, but the densities of the two systems, that is, the quantities of matter contained in an equal bulk or extent, be different, it is evident that the quantities of motion produced in the two systems in the same time will be proportional to the densities; and if the densities are the same, and uniform in each system, the quantities of motion produced will be as the squares of the velocities, because the motion communicated to each corresponding body will be proportional to the velocity communicated, that is, to the velocity of the impelling body; and the number of similarly situated particles which will be agitated will also be proportional to this velocity. Therefore, the whole quantities of motion produced in the same moment of time will be proportional to the squares of the velocities. And, lastly, if the densities of the two systems are uniform, or the same through the whole extent of the systems, the number of particles impelled by similar bodies will be as the surfaces of these bodies.

Now the diminutions of the motions of the projected bodies are (by Newton's third law of motion) equal to the motions produced in the systems; and these diminutions are the measures of what are called the resistances opposed to the motions of the projected bodies. Therefore, combining all these circumstances, the resistances are proportional to the similar surfaces of the moving bodies, to the densities of the systems through which the motions are performed, and to the squares of the velocities, jointly.

We cannot form to ourselves any distinct notion of a fluid, otherwise than as a system of small bodies, or a collection of particles, similarly or symmetrically arranged, the centres of each being situated in the angles of regular solids. We must form this notion of it, whether we suppose, with the vulgar, that the particles are little globules in mutual contact, or, with the partisans of corpuscular attractions and repulsions, we suppose the particles kept at a distance from each other by means of these attractions and repulsions mutually balancing each other. In this last case, no other arrangement is consistent with a quiescent equilibrium; and in this case, it is evident, from the theory of curvilineal motions, that the agitations of the particles will always be such, that the connecting forces, in actual exertion, will be proportional to the squares of the velocities directly, and to the chords of the curvature having the direction of the forces inversely.

PROP. I. The resistances, and (by the third law of motion), the impulsions of fluids on similar bodies, are proportional to the surfaces of the solid bodies, to the densities of the fluids, and to the squares of the velocities, jointly.

We must now observe, that when we suppose the particles of the fluid to be in mutual contact, we may either suppose them elastic or unelastic. The motion communicated to the collection of elastic particles must be double of what the same body, moving in the same manner, would communicate to the particles of an elastic fluid. The impulse and resistance of elastic fluids must therefore be double of those of unelastic fluids.—But we must caution our readers not to judge of the elasticity of fluids by their sensible compressibility. A diamond is incomparably more elastic than the finest foot-ball, though not compressible in any sensible degree.—It remains to be decided, by well-chosen experiments, whether water be not as elastic as air. If we suppose, with Boscovich, the particles of perfect fluids to be at a distance from each other, we shall find it difficult to conceive a fluid void of elasticity. We hope that the theory of their impulse and resistance will suggest experiments which will decide this question, by pointing out what ought to be the absolute impulse or resistance in either case. And thus the fundamental proposition of the impulse and resistance of fluids, taken in its proper meaning, is susceptible of a rigid demonstration, relative to the only distinct notion that we can form of the internal constitution of a fluid. We say, *taken in its proper meaning*; namely, that the impulse or resistance of fluids is a pressure, opposed and measured by another pressure, such as a pound weight, the force of a spring, the pressure of the atmosphere, and the like. And we apprehend that it would be very difficult to find any legitimate demonstration of this leading proposition different from this, which we have now borrowed from Sir Isaac Newton, Prop. 23. B. II. *Princip.* We acknowledge that it is prolix and even circuitous: but in all the attempts made by his commentators and their copyists to simplify it, we see great defects of logical argument, or assumption of principles, which are not only gratuitous, but inadmissible. We shall have occasion, as we proceed, to point out some of these defects; and doubt not but the illustrious author of this demonstration had exercised his uncommon patience and sagacity in similar attempts, and was dissatisfied with them all.

Before we proceed farther, it will be proper to make a general remark, which will save a great deal of discussion. Since it is a matter of universal experience, that every action of a body on others is accompanied by an equal and contrary re-action; and since all that we can demonstrate concerning the resistance of bodies, during their motions through fluids, proceeds on this supposition (the resistance of the body being *assumed* as equal and opposite to the sum of motions communicated to the particles of the fluid, estimated in the direction of the bodies' motion), we are entitled to proceed in the contrary order, and to consider the impulsions which each of the particles of fluid exerts on the body at rest, as equal and opposite to the motion which the body would communicate to that particle if the fluid were at rest, and the body were moving equally swift in the opposite direction; and therefore the whole impulsion of the fluid must be conceived as the measure of the whole motion which the body would thus communicate to the fluid. It must therefore be also considered as the measure of the resistance which the body, moving with the same velocity, would sustain from the fluid. When, therefore, we shall demonstrate any thing concerning the impulsion of a fluid, estimated in the direction of its motion, we must consider it as demonstrated concerning the resistance of a quiescent fluid to the motion of that body, having the same velocity in the opposite direction. The determination of these impulsions being much easier than the determination of the motions communicated by the body to the particles of the fluid, this method will be followed in most of the subsequent discussions.

The general proposition already delivered is by means sufficient for explaining the various important phenomena observed in the mutual actions of solids and fluids. In particular, it gives us no assistance in ascertaining the modifications of this resistance or impulse, which depend on the shape of the body, and the inclination of its impelled or resisted surface to the direction of the motion. Sir Isaac Newton found another hypothesis necessary; namely, that the fluid should be so extremely rare, that the distance of the particles may be incomparably greater than their diameters. This additional condition is necessary for considering their actions as so many separate collisions or impulsions on a solid body. Each particle must be supposed to have abundant room to rebound, or otherwise escape, after having made its stroke, without sensibly affecting the situations and motions of the particles which have not yet made their stroke; and the motion must be so swift as not to give time for the sensible exertion of their mutual forces of attractions and repulsions.

Keeping these conditions in mind, we may proceed to determine the impulsions made by a fluid on surfaces of every kind; and the most convenient method to pursue in this determination, is to compare them all either with the impulse which the *same surface* would receive from the fluid impinging on it perpendicularly, or with the impulse which the *same stream of fluid* would make when coming perpendicularly on a surface of such extent as to occupy the whole stream.

It will greatly abbreviate language, if we make use of a few terms in an appropriated sense.

By a *stream*, we shall mean a quantity of fluid moving in one direction, that is, each particle moving in parallel lines; and the *breadth* of the stream is a line perpendicular to all these parallels.

A *filament* means a portion of this stream of very small breadth, and it consists of an indefinite number of particles following one another in the same direction, and successively impinging on, or gliding along, the surface of the solid body.

The *base* of any surface exposed to a stream of fluid, is that portion of a plane perpendicular to the stream, which is covered or protected from the action of the stream by the surface exposed to its impulse. Thus the base of a sphere exposed to a stream of fluid is its great circle, whose plane is perpendicular to the stream. If BC (Fig. 1.) be a plane surface exposed to the action of a stream of fluid, moving in the direction DC, then BR, or SE, perpendicular to DC, is its base.

Direct impulse shall express the energy or action of the particle or filament, or stream of fluid, when meeting the surface perpendicularly, or when the surface is perpendicular to the direction of the stream.

Absolute impulse means the actual pressure on the impelled surface, arising from the action of the fluid, whether striking the surface perpendicularly or obliquely; or it is the force impressed on the surface, or tendency to motion which it acquires, and which must be opposed by an equal force in the opposite direction, in order that the surface may be maintained in its place. It is of importance to keep in mind, that this pressure is always perpendicular to the surface. It is a proposition founded on universal and uncontradicted experience, that the mutual actions of bodies on each other are always exerted in a direction perpendicular to the touching surfaces. Thus, it is observed, that when a billiard-ball A is struck by another B, moving in any direction whatever, the ball A always moves off in the direction perpendicular to the plane which touches the two balls in the point of mutual contact, or point of impulse. This inductive proposition is supported by every argument which can be drawn from what we know concerning the forces which connect the particles of matter together, and are the immediate causes of the communication of motion. It would employ much time and room to state them here; and we apprehend that it is unnecessary: for no reason can be assigned why the pressure should be in *any particular* oblique direction. If any one should say that the impulse will be in the direction of the stream, we have only to desire him to take notice of the effect of the rudder of a ship. This shows that the impulse *is not in the direction of the stream*, and is therefore in some direction transverse to the stream.—He will also find, that when a plane surface is impelled obliquely by a fluid, there is no direction in which it can be supported but the direction perpendicular to itself. It is quite safe, in the mean time, to take it as an experimental truth. We may, perhaps, in some other part of this work, give what will be received as a rigorous demonstration.

Relative or effective impulse means the pressure on the surface estimated in some particular direction. Thus, BC (Plate IX. Fig. 1.) may represent the sail of a ship, impelled by the wind blowing in the direction DC. GO may be the direction of the ship's keel, or the line of her course. The wind strikes the sail in the direction GH parallel to DC; the sail is urged or pressed in the direction GI, perpendicular to BC. But we are interested to know what tendency this will give the ship to move in the direction GO. This is the effective or relative impulse. Or BC may be the transverse section of the sail of a common wind-mill. This, by the construction of the machine, can move only in the direction GP, perpendicular to the direction of the wind; and it is only in this direction that the impulse produces the desired effect. Or BC may be half of the prow of a punt or lighter, riding at anchor by means of the cable DC, attached to the prow C. In this case, GQ, parallel to DC, is that part of the absolute impulse which is employed in straining the cable.

The *angle of incidence* is the angle FGC contained between the direction of the stream FG and the plane BC.

The *angle of obliquity* is the angle OGC contained between the plane and the direction GO, in which we wish to estimate the impulse.

Prop. II. The direct impulse of a fluid on a plane surface, is to its absolute oblique impulse on the same surface, as the square of the radius to the square of the sine of the angle of incidence.

Let a stream of fluid, moving in the direction DC, (Fig. 1.) act on the plane BC. With the radius CB describe the quadrant ABE; draw CA perpendicular to CE, and draw MNBS parallel to CE. Let the particle F, moving in the direction FG, meet the plane in G, and in FG produced take GH to represent the magnitude of the direct impulse, or the impulse which the particle would exert on the plane AC, by meeting it in V. Draw GI and HK perpendicular to BC, and HI perpendicular to GI. Also draw BR perpendicular to DC.

The force GH is equivalent to the two forces GI and GK; and GK, being in the direction of the plane, has no share in the impulse. The absolute impulse, therefore, is represented by GI; the angle GHI is equal to FGC, the angle of incidence; and therefore GH is to GI as radius to the sine of the angle of incidence: Therefore the direct impulse of each particle or filament is to its absolute oblique impulse as radius to the sine of the angle of incidence. But further, the number of particles or filaments which strike the surface AC, is to the number of those which strike the surface BC as AC to NC; for all the filaments between LA and MB go past the oblique surface BC without striking it. But BC : NC = rad. : sin. NBC, = rad. :

sin. FGC, = rad. : sin. incidence. Now the whole impulse is as the impulse of each filament, and as the number of filaments exerting equal impulses jointly; therefore the whole direct impulse on AC is to the whole absolute impulse on BC, as the square of radius to the square of the sine of the angle of incidence.

Let S express the extent of the surface, i the angle of incidence, o the angle of obliquity, v the velocity of the fluid, and d its density. Let F represent the direct impulse, f the absolute oblique impulse, and φ the relative or effective impulse; and let the tabular sines and cosines be considered as decimal fractions of the radius unity.

This proposition gives us $F : f = R^2 : \sin.^2 i, = 1 : \sin.^2 i$, and therefore $f = F \times \sin.^2 i$. Also, because impulses are in the proportion of the extent of surface similarly impelled, we have, in general, $f = FS \times \sin.^2, i$.

The first who published this theorem was Pardies, in his *Oeuvres de Mathematique*, in 1673. We know that Newton had investigated the chief propositions of the Principia before 1670.

PROP. III. The direct impulse on any surface is to the effective oblique impulse on the same surface, as the cube of radius to the solid, which has for its base the square of the sine of incidence, and the sine of obliquity for its height.

For when GH represents the direct impulse of a particle, GI is the absolute oblique impulse, and GO is the effective impulse in the direction GO: Now GI is to GO as radius to the sine of GIO, and GIO is the complement of IGO, and is therefore equal to CGO, the angle of obliquity.

Therefore $f : \varphi = R : \sin. O$.
But $F : f = R^2 : \sin.^2 i$
Therefore $F : \varphi = R^3 : \sin.^2 i \times \sin. O$, and $\varphi = F \times \sin.^2 i \times \sin. O$.

Cor.—The direct impulse on any surface is to the effective oblique impulse in the direction of the stream, as the cube of radius to the cube of the sine of incidence. For draw IQ and GP perpendicular to GH, and IP perpendicular to GP; then the absolute impulse GI is equivalent to the impulse GQ in the direction of the stream, and GP, which may be called the transverse impulse. The angle GIQ is evidently equal to the angle GHI, or FGC, the angle of incidence.

Therefore $f : \varphi = \mathrm{GI} : \mathrm{GQ}, = \mathrm{R} : \sin. i.$
But $\mathrm{F} : f = \mathrm{R}^2 : \sin.^2 i.$
Therefore $\mathrm{F} : \varphi = \mathrm{R}^3 : \sin.^3 i.$
And $\varphi = \mathrm{F} \times \sin.^3 i.$

Before we proceed further, we shall consider the impulse on a surface which is also in motion. This is evidently a frequent and an important case. It is perhaps the most frequent and important: It is the case of a ship under sail, and of a wind or water-mill at work.

Therefore, let a stream of fluid, moving with the direction and velocity DE, meet a plane BC, (Fig. 2.) which is moving parallel to itself in the direction and with the velocity DF: It is required to determine the impulse?

Nothing is more easy. The mutual actions of bodies depend on their relative motions only. The motion DE of the fluid relative to BC, which is also in motion, is compounded of the real motion of the fluid, and the opposite to the real motion of the body. Therefore produce FD till D*f* = DF, and complete the parallelogram D*fe*E, and draw the diagonal D*e*. The impulse on the plane is the same as if the plane were at rest, and every particle of the fluid impelled it in the direction and with the velocity D*e*; and may therefore be determined by the foregoing proposition. This proposition applies to every possible case; and we shall not bestow more time on it, but reserve the important modification of the general proposition for the cases which shall occur in the practical applications of the whole doctrine of the impulse and resistance of fluids.

Prop. IV. The direct impulse of a stream of fluid, whose breadth is given, is to its oblique effective impulse in the direction of the stream, as the square of radius to the square of the sine of the angle of incidence.

For the number of filaments which occupy the oblique plane BC, would occupy the portion NC of a perpendicular plane, and therefore we have only to compare the perpendicular impulse on any point V with the effective impulse made by the same filament FV on the oblique plane at G. Now GH represents the impulse which this filament would make at V; and GQ is the effective impulse of the same filament at G, estimated in the direction GH of the stream; and GH is to GQ as GH^2 to GI^2, that is, as rad.^2 to $\sin.^2 i$.

Cor. 1. The effective impulse in the direction of the stream on any plane surface BC, is to the direct impulse on its base BR or SE, as the square of the sine of the angle of incidence to the square of the radius.

2. If an isosceles wedge ACB (Fig. 3.) be exposed to a stream of fluid moving in the direction of its height CD, the impulse on the sides is to the direct impulse on the base, as the square of half the base AD to the square of the side AC, or as the square of the sine of half the angle of the wedge to the square of the radius. For it is evident, that, in this case, the two transverse impulses, such as GP in Fig. 1. balance each other; and the only impulse which can be observed is the sum of the two impulses, such as GQ of Fig. 1. which are to be compared with the impulses on the two halves AD, BD of the base. Now AC : AB = rad. : sin. ACD, and ACD is equal to the angle of incidence.

Therefore, if the angle ACB is a right angle, and ACD is half a right angle, the square of AC is twice the square of AD, and the impulse on the sides of a rectangular wedge is half the impulse on its base.

Also, if a cube ACBE (Fig. 4.) be exposed to a stream moving in a direction perpendicular to one of its sides, and then to a stream moving in a direction perpendicular to one of its diagonal planes, the impulse in the first case will be to the impulse in the second as $\sqrt{2}$ to 1. Call the perpendicular impulse on a side F, and the perpendicular impulse on its diagonal plane f, and the effective oblique impulse on its sides φ;—we have

$$F : f = AC : AB = 1 : \sqrt{2}, \text{ and}$$

$$f : \varphi = AC^2 : AD^2 = 2 : 1. \text{ Therefore}$$

$$F : \varphi = \quad 2 : \sqrt{2}, = \sqrt{2} : 1,$$ or very nearly as 10 to 7.

The same reasoning will apply to a pyramid whose base is a regular polygon, and whose axis is perpendicular to the base. If such a pyramid is exposed to a stream of fluid moving in the direction of the axis, the direct impulse on the base is to the effective impulse on the pyramid, as the square of the radius to the square of the sine of the angle which the axis makes with the sides of the pyramid.

And, in like manner, the direct impulsion on the base of a right cone is to the effective impulsion on the conical surface, as the square of the radius to the square of the sine of half the angle at the vertex of the cone. This is demonstrated, by supposing the cone to be a pyramid of an infinite number of sides.

We may in this manner compare the impulse on any polygonal surface with the impulse on its base, by comparing apart the impulses on each plane with those in their corresponding bases, and taking their sum.

And we may compare the impulse on a curved surface with that on its base, by resolving the curved surface into elementary planes, each of which is impelled by an elementary filament of the stream.

The following beautiful proposition, given by Le Seur and Jaquier, in their Commentary on the Second Book of Newton's Principia, with a few examples of its application, will suffice for any further account of this theory.

PROP. V. Let ADB (Fig. 5.) be the section of a surface of simple curvature, such as is the surface of a cylinder. Let this be exposed to the action of a fluid moving in the direction AC. Let BC be the section of the plane (which we have called its base), perpendicular to the direction of the stream. In AC produced, take any length CG; and on CG describe the semicircle CHG, and complete the rectangle BCGO. Through any point D of the curve draw ED parallel to AC, and meeting BC and OG in Q and P. Let DF touch the curve in D, and draw the chord GH parallel to DF, and HKM perpendicular to CG, meeting ED in M. Suppose this to be done for every point of the curve ADB, and let LMN be the curve which passes through all the points of intersection of the parallels EDP and the corresponding perpendiculars HKM.

The effective impulse on the curve surface ADB in the direction of the stream, is to its direct impulse on the base BC as the area BCNL is to the rectangle BCGO. (Fig. 4.)

Draw *edqmp* parallel to EP, and extremely near it. The arch D*d* of the curve may be conceived as the section of an elementary plane, having the position of the tangent DF. The angle EDF is the angle of incidence of the filament ED*de*. This is equal to CGH, because ED, DF, are parallel to CG, GH; and (because CHG is a semicircle) CH is perpendicular to GH. Also, CG : CH = CH : CK, and CG : CK = CG^2 : CH^2, = rad.^{2} : sin.^{2}, CGH, = rad.^{2} : sin.^{2} incid. Therefore, if CG, or its equal DP, represent the direct impulse on the point Q of the base, CK, or its equal QM, will represent the effective impulse on the point D of the curve. And thus, Q*qp*P will represent the direct impulse of the filament on the element Q*q* of the base, and Q*qm*M will represent the effective impulse of the same filament on the element D*d* of the curve. And, as this is true of the whole curve ADB, the effective impulse on the whole curve will be represented by the area BCNML; and the direct impulse on the base will be represented by the rectangle BCGO; and therefore the impulse on the curve-surface is to the impulse on the base as the area BLMNC is to the rectangle BOGC.

It is plain, from the construction, that if the tangent to the curve at A is perpendicular to AC, the point N will coincide with G. Also, if the tangent to the curve at B is parallel to AC, the point L will coincide with B.

Whenever, therefore, the curve ADB is such, that an equation can be had to exhibit the general relation between the abscissa AR and the ordinate DR, we shall deduce an equation which exhibits the relation between the absciss CK and the ordinate KM of the curve LMN; and this will give us the ratio of BLNC to BOGC.

Thus, if the surface is that of a cylinder, so that the curve BDA*b*, (Fig. 6.) which receives the impulse of the fluid, is a semicircle, make CG equal to AC, and construct the figure as before. The curve BMG is a parabola, whose axis CG, whose vertex is G, and whose parameter is equal to CG. For it is plain, that $CG = DC$, (Fig. 4.) and $GH = CQ, = MK$. And $CG \times GK = GH^2 = KM^2$; that is, the curve is such, that the square of the ordinate KM is equal to the rectangle of the abscissa GK and a constant line GC; and it is therefore a parabola, whose vertex is G. Now, it is well known, that the parabolic area BMGC is two-thirds of the parallelogram BCGO. Therefore the impulse on the quadrant ADB is two-thirds of the impulse on the base BC. The same may be said of the quadrant A *d b* and its base *c b*. Therefore, *The impulse on a cylinder or half cylinder is two-thirds of the direct impulse on its transverse plane through the axis*; or it is two-thirds of the direct impulse on one side of a parallelopiped of the same breadth and height.

Prop. VI. If the body be a solid generated by the revolution of the figure BDAC (Fig. 5.) round the axis AC, and if it be exposed to the action of a stream of fluid moving in the direction of the axis AC; then the effective impulse in the direction of the stream is to the direct impulse on its base, as the solid generated by the revolution of the figure BLMNC round the axis CN to the cylinder generated by the revolution of the rectangle BOGC.

This scarcely needs a demonstration. The figure ADBLMNA is a section of these solids by a plane passing through the axis; and what has been demonstrated of this section is true of every other, because they are all equal and similar. It is therefore true of the whole solids, and (their base) the circle generated by the revolution of BC round the axis AC.

Hence we easily deduce, that *The impulse on a sphere is one half of the direct impulse on its great circle, or on the base of a cylinder of equal diameter.*

For, in this case, the curve BMN, (Fig. 6.) which generates the solid expressing the impulse on the sphere, is a parabola, and the solid is a parabolic conoid. Now this conoid is to the cylinder generated by the revolution of the rectangle BOGC round the axis CG, as the sum of all the circles generated by the revolution of ordinates to the parabola such as KM, to the sum of as many circles generated by the ordinates to the rectangle such as KT; or as the sum of all the squares described on the ordinates KM to the sum of as many squares described on the ordinates KT. Draw BG cutting MK in S. The square on MK is to the square on BC or TK as the abscissa GK to the abscissa GC (by the nature of the parabola), or as SK to BC; because SK and BC are respectively equal to GK and GC. Therefore the sum of all the squares on ordinates, such as MK, is to the sum of as many squares on ordinates, such as TK, as the sum of all the lines SK to the sum of as many lines TK; that is, as the triangle BGC to the rectangle BOGC; that is, as one to two: and therefore the impulse on the sphere is one-half of the direct impulse on its great circle.

From the same construction we may very easily deduce a very curious and seemingly useful truth, that of all conical bodies having the circle whose diameter is AB (Fig. 3.) for its base, and FD for its height, the one which sustains the smallest impulse, or meets with the smallest resistance, is the frustum AGHB of a cone ACB so constructed, that EF being taken equal to ED, EA is equal to EC. This frustum, though more capacious than the cone AFB of the same height, will be less resisted.

Also, if the solid generated by the revolution of BDAC (Fig. 5.) have its anterior part covered with a frustum of a cone generated by the lines D *a*, *a* A, forming the angle at *a* of 135 degrees; this solid, though more capacious than the included solid, will be less resisted.

And, from the same principles, Sir Isaac Newton determined the form of the curve ADB, which would generate the solid which, of all others of the same length and base, should have the least resistance.

These are curious and important deductions, but are not introduced here, for reasons which will soon appear.

The reader cannot fail to observe, that all that we have hitherto delivered on this subject, relates to the comparison of different impulses or resistances. We have always compared the oblique impulsions with the direct, and by their intervention we compare the oblique impulsions with each other. But it remains to give absolute measures of some individual impulsion; to which, as to an unit, we may refer every other. And as it is by their pressure that they become useful or hurtful, and they must be opposed by other pressures, it becomes extremely convenient to compare them all with that pressure with which we are most familiarly acquainted, the pressure of gravity.

The manner in which the comparison is made is this. When a body advances in a fluid with a known velocity, it puts a known quantity of the fluid into motion (as is supposed) with this velocity; and this is done in a known time. We have only to examine what weight will put this quantity of fluid into the same motion, by acting on it during the same time. This weight is conceived as equal to the resistance. Thus, let us suppose that a stream of water, moving at the rate of eight feet per second, is perpendicularly obstructed by a square foot of solid surface held fast in its place. Conceiving water to act in the manner of the hypothetical fluid now described, and to be without elasticity, the whole effect is the gradual annihilation of the motion of eight cubic feet of water moving eight feet in a second. And this is done in a second of time. It is equivalent to the gradually putting eight cubic feet of water into motion with this velocity; and doing this by acting uniformly during a second. What weight is able to produce this effect? The weight of eight feet of water, acting during a second on it, will, as is well known, give it the velocity of thirty-two feet per second; that is, four times greater. Therefore, the weight of the fourth part of eight cubic feet, that is, the weight of two cubic feet, acting during a second, will do the same thing, or the weight of a column of water whose base is a square foot, and whose height is two feet. This will not only produce this effect in the same time with the impulsion of the solid body, but it will also do it by the same degrees, as any one will clearly perceive, by attending to the gradual acceleration of the mass of water urged by one-fourth of its weight, and comparing this with the gradual production or extinction of motion in the fluid by the progress of the resisted surface.

Now it is well known that eight cubic feet of water, by falling one foot, which it will do in one-fourth of a second, will acquire the velocity of eight feet per second by its weight; therefore the force which produces the same effect in a whole second is one-fourth of this. This force is therefore equal to the weight of a column of water, whose base is a square foot, and whose height is two feet; that is, twice the height necessary for acquiring the velocity of the motion by gravity. The conclusion is the same whatever be the surface that is resisted, whatever be the fluid that resists, and whatever be the velocity of the motion. In this inductive and familiar manner we learn, that *the direct impulse or resistance of an unelastic fluid on any plane surface, is equal to the weight of a column of the fluid having the surface for its base, and twice the fall necessary for acquiring the velocity of the motion for its height:* and if the fluid is considered as elastic, the impulse or resistance is twice as great. See Newt. *Princip.* B. II. Prop. 35. and 38.

It now remains to compare this theory with experiment. Many have been made, both by Sir Isaac Newton and by subsequent writers. It is much to be lamented, that in a matter of such importance, both to the philosopher and to the artist, there is such a disagreement in the results with each other. We shall mention the experiments which seem to have been made with the greatest judgment and care. Those of Sir Isaac Newton were chiefly made by the oscillations of pendulums in water, and by the descent of balls both in water and in air. Many have been made by Mariotte (*Traité de Mouvement des Eaux*). Gravesande has published, in his *System of Natural Philosophy*, experiments made on the resistance or impulsions on solids in the midst of a pipe or canal. They are extremely well contrived, but are on so small a scale that they are of very little use. Daniel Bernoulli, and his pupil Professor Krafft, have published in the *Comment. Acad. Petropol.* experiments on the impulse of a stream or vein of water from an orifice or tube. These are of great value. The Abbé Bossut has published others of the same kind in his *Hydrodynamique.* Mr Robins has published, in his *New Principles of Gunnery*, many valuable experiments on the impulse and resistance of air. The Chev. de Borda, in the *Mem. Acad. Paris*, 1763 and 1767, has given experiments on the resistance of air and also of water, which are very interesting.

The most complete collection of experiments on the resistance of water are those made at the public expense by a committee of the academy of sciences, consisting of the Marquis de Condorcet, Mr D'Alembert, Abbé Bossut, and others. The Chev. de Buat, in his *Hydraulique*, has published some most curious and valuable experiments where many important circumstances are taken notice of, which had never been attended to before, and which give a view of the subject totally different from what is usually taken of it. Don George d'Ulloa, in his *Examine Maritimo*, has also given some important experiments, similar to those adduced by Bouguer in his *Manœuvre des Vaisseaux*, but leading to very different conclusions. All these should be consulted by such as would acquire a practical knowledge of this subject. We must content ourselves with giving their most general and steady results. Such as,

1. It is very consonant to experiment that the resistances are proportional to the squares of the velocities. When the velocities of water do not exceed a few feet per second, no sensible deviation is observed. In very small velocities the resistances are sensibly greater than in this proportion, and this excess is plainly owing to the viscidity or imperfect fluidity of water. Sir Isaac Newton has shown that the resistance arising from this cause is constant, or the same in every velocity; and when he has taken off a certain part of the total resistance, he found the remainder was very exactly proportionable to the square of the velocity. His experiments to this purpose were made with balls a very little heavier than water, so as to descend very slowly; and they were made with his usual care and accuracy, and may be depended on.

In the experiments made with bodies floating on the surface of water, there is an addition to the resistance arising from the inertia of the water. The water heaps up a little on the anterior surface of the floating body, and is depressed behind it. Hence arises a hydrostatical pressure, acting in concert with the true resistance. A similar thing is observed in the resistance of air, which is condensed before the body and rarefied behind it, and thus an additional resistance is produced by the unbalanced elasticity of the air; and also because the air, which is *actually* displaced, is denser than common air. These circumstances cause the resistances to increase faster than the squares of the velocities: but, even independent of this, there is an additional resistance arising from the tendency to rarefaction behind a very swift body; because the pressure of the surrounding fluid can only make the fluid fill the space left with a determined velocity.

We have had occasion to speak of this circumstance more particularly under GUNNERY and PNEUMATICS, when considering very rapid motions. Mr Robins had remarked that the velocity at which the observed resistance of the air began to increase so prodigiously, was that of about 1100 or 1200 feet per second, and that this was the velocity with which air would rush into a void. He concluded, that when the velocity was greater than this, the ball was exposed to the additional resistance arising from the unbalanced statical pressure of the air, and that this constant quantity behoved to be added to the resistance arising from the air's inertia in all greater velocities. This is very reasonable: But he imagined that in smaller velocities there was no such unbalanced pressure. But this cannot be the case: for although in smaller velocities the air will still fill up the space behind the body, it will not fill it up with air of the same density. This would be to suppose the motion of the air into the deserted place to be instantaneous. There must therefore be a rarefaction behind the body, and a pressure backward; arising from unbalanced elasticity, independent of the condensation on the anterior part. The condensation and rarefaction are caused by the same thing, viz. the limited elasticity of the air. Were this infinitely great, the smallest condensation before the body would be instantly diffused over the whole air, and so would the rarefaction, so that no pressure of unbalanced elasticity would be observed; but the elasticity is such as to propagate the condensation with the velocity of sound only, *i. e.* the velocity of 1142 feet per second. Therefore this additional resistance does not commence precisely at this velocity, but is sensible in all smaller velocities, as is very justly observed by Euler. But we are not yet able to ascertain the law of its increase, although it is a problem which seems susceptible of a tolerably accurate solution.

Precisely similar to this is the resistance to the motion of floating bodies, arising from the accumulation or gorging up of the water on their anterior surface, and its depression behind them. Were the gravity of the water infinite, while its inertia remains the same, the wave raised up at the prow of a ship would be instantly diffused over the whole ocean, and it would therefore be infinitely small, as also the depression behind the poop. But this wave requires time for its diffusion; and while it is not diffused, it acts by hydrostatical pressure. We are equally unable to ascertain the law of variation of this part of the resistance, the mechanism of waves being but very imperfectly understood. The height of the wave in the experiments of the French Academy could not be measured with sufficient precision (being only observed *en passant*) for ascertaining its relation to the velocity. The Chev. Buat attempted it in his experiments, but without success. This must evidently make a part of the resistance in all velocities: and it still remains an undecided question, "What relation it bears to the velocities?" When the solid body is wholly buried in the fluid, this accumulation does not take place, or at least not in the same way: It may, however, be observed. Every person may recollect, that in a very swift running stream a large stone at the bottom will produce a small swell above it; unless it lies very deep, a nice eye may still observe it. The water, on arriving at the obstacle, glides past it in every direction, and is deflected on all hands; and therefore what passes over it is also deflected upwards, and causes the water over it to rise above its level. The nearer that the body is to the surface, the greater will be the perpendicular rise of the water, but it will be less diffused; and it is uncertain whether the *whole* elevation will be greater or less. By the whole elevation we mean the area of a perpendicular section of the elevation by a plane perpendicular to the direction of the stream. We are rather disposed to think that this area will be greatest when the body is near the surface. D'Ulloa has attempted to consider this subject scientifically; and is of a very different opinion, which he confirms by the single experiment to be mentioned by and by. Mean time, it is evident, that if the water which glides past the body cannot fall in behind it with sufficient velocity for filling up the space behind, there must be a void there; and thus a hydrostatical pressure must be superadded to the resistance arising from the inertia of the water. All must have observed, that if the end of a stick held in the hand be drawn slowly through the water, the water will fill the place left by the stick, and there will be no curled wave: but if the motion be very rapid, a hollow trough or gutter is left behind, and is not filled up till at some distance from the stick, and the wave which forms its sides is very much broken and curled. The writer of this article has often looked into the water from the poop of a second-rate man of war when she was sailing 11 miles per hour, which is a velocity of 16 feet per second nearly; and he not only observed that the back of the rudder was naked for about two feet below the load water-line, but also that the trough or wake made by the ship was filled up with water which was broken and foaming to a considerable depth, and to a considerable distance from the vessel: There must therefore have been a void. He never saw the wake perfectly transparent (and therefore completely filled with water) when the velocity exceeded 9 or 10 feet per second. While this broken water is observed, there can be no doubt that there is a void and an additional resistance. But even when the space left by the body, or the space behind a still body exposed to a stream, is completely filled, it may not be filled sufficiently fast, and there may be (and certainly is, as we shall see afterwards) a quantity of water behind the body, which is moving more slowly away than the rest, and therefore hangs in some shape by the body, and is dragged by it, increasing the resistance. The quantity of this must depend partly on the velocity of the body or stream, and partly on the rapidity with which the surrounding water comes in behind. This last must depend on the pressure of the surrounding water. It would appear, that when this adjoining pressure is very great, as must happen when the depth is great, the augmentation of resistance now spoken of would be less. Accordingly, this appears in Newton's experiments, where the balls were less retarded as they were deeper under water.

These experiments are so simple in their nature, and were made with such care, and a person so able to detect and appreciate every circumstance, that they deserve great credit; and the conclusions *legitimately* drawn from them deserve to be considered as physical laws. We think that the present deduction is unexceptionable: for in the motion of balls, which hardly descended, their preponderancy being hardly sensible, the effect of depth must have borne a very great proportion to the whole resistance, and must have greatly influenced their motions; yet they were observed to fall as if the resistance had no way depended on the depth.

The same thing appears in Borda's experiments, where a sphere which was deeply immersed in the water was less resisted than one that moved with the same velocity near the surface; and this was very constant and regular in a course of experiments. D'Ulloa, however, affirms the contrary: He says that the resistance of a board, which was a foot broad, immersed one foot in a stream moving two feet per second, was 15½lbs.; and the resistance to the same board, when immersed 2 feet in a stream moving $1\frac{1}{3}$ feet per second (in which case the surface was 2 feet), was $26\frac{1}{4}$ pounds.*

We are very sorry that we cannot give a proper account of this theory of resistance by Don George Juan D'Ulloa, an author of great mathematical reputation, and the inspector of the marine academies in Spain. We have not been able to procure either the original or the French translation, and judge of it only by an extract by Mr Prony in his *Architecture Hydraulique*, sect. 868, &c. The theory is enveloped (according to Mr Prony's custom) in the most complicated expressions, so that the physical principles are kept almost out of sight. When accommodated to the simplest possible case, it is nearly as follows:

Let o be an elementary orifice or portion of the surface of the side of a vessel filled with a heavy fluid, and let h be its depth under the horizontal surface of the fluid. Let δ be the density of the fluid, and φ the accelerative power of gravity, = 32 feet velocity acquired in a second.

It is known, says he, that the water would flow out at this hole with the velocity $u = \sqrt{2\varphi h}$, and $u^2 = 2\varphi h$ and $h = \frac{u^2}{2\varphi}$. It is also known that the pressure p on the orifice o is $\varphi o \delta h, = \varphi o \delta \frac{u^2}{2\varphi} = \frac{1}{2} \delta o u^2$.

Now, let this little surface o be supposed to move with the velocity v. The fluid would meet it with the velocity $u + v$, or $u - v$, according as it moved in the opposite or in the same direction with the efflux. In the equation $p = \frac{1}{2} \delta o u^2$, substitute $u \pm v$ for u, and we have the pressure on $o = p = \frac{\delta o}{2} (u \pm v)^2, = \frac{\delta o}{2} (\sqrt{2\varphi h} \pm v^2)$.

This pressure is a weight, that is, a mass of matter m

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* There is something very unaccountable in these experiments. The resistances are much greater than any other author has observed.

actuated by gravity φ, or $p = \varphi m$, and $m = \partial o \left(\sqrt{h} \pm \frac{v}{\sqrt{2\varphi}}\right)^2$.

This elementary surface being immersed in a stagnant fluid, and moved with the velocity v, will sustain on one side a pressure $\partial o \left(\sqrt{h} + \frac{v}{\sqrt{2\varphi}}\right)^2$, and on the other side a pressure $\partial o \left(\sqrt{h} - \frac{v}{\sqrt{2\varphi}}\right)^2$; and the sensible resistance will be the difference of these two pressures, which is $\partial o\, 4 \sqrt{h} \frac{v}{\sqrt{2\varphi}}$, or $\partial o\, 4 \sqrt{h} \frac{v}{8}$, that is $\frac{\partial o \sqrt{h}\, v}{2}$ because $\sqrt{2\varphi} = 8$; a quantity which is in the subduplicate ratio of the depth under the surface of the fluid, and the simple ratio of the velocity of the resisted surface jointly.

There is nothing in experimental philosophy more certain than that the resistances are very nearly in the duplicate ratio of the velocities; and we cannot conceive by what experiments the ingenious author has supported this conclusion.

But there is, besides, what appears to us to be an essential defect in this investigation. The equation exhibits no resistance in the case of a fluid without weight. Now a theory of the resistance of fluids should exhibit the retardation arising from inertia alone, and should distinguish it from that arising from any other cause: and moreover, while it *assigns* an ultimate sensible resistance proportional (*cæteris paribus*) to the simple velocity, it *assumes* as a first principle that the pressure p is as $\overline{u \pm v}^2$. It also gives a false measure of the statical pressures; for these (in the case of bodies immersed in our waters at least) are made up of the pressure of the incumbent water, which is measured by h, and the pressure of the atmosphere, a constant quantity.

Whatever reason can be given for setting out with the principle that the pressure on the little surface o, moving with the velocity u, is equal to $\frac{1}{2}\partial\, o\, (u \pm v)^2$, makes it indispensably necessary to take for the velocity u, not that with which water would issue from a hole whose depth under the surface is h, but the velocity with which it will issue from a hole whose depth is $h + 33$ feet. Because the pressure of the atmosphere is equal to that of a column of water 33 feet high; for this is the acknowledged velocity with which it would rush into the void left by the body. If, therefore, this velocity (which does not exist) has any share in the effort, we must have for the fluxion of pressure not $\frac{4\sqrt{h}\,v}{\sqrt{2\phi}}$, but $\frac{4\sqrt{h+33}\,v}{\sqrt{2\phi}}$. This would not only give pressure or resistances many times exceeding those that have been observed in our experiments, but would also totally change the proportions which this theory determines. It was at any rate improper to embarrass an investigation, already very intricate, with the pressure of gravity, and with two motions of efflux, which do not exist, and are necessary for making the pressures in the ratio of $\overline{u+v}^2$ and $\overline{u-v}^2$.

Mr Prony has been at no pains to inform his readers of his reasons for adopting this theory of resistance, so contrary to all received opinions, and to the most distinct experiments. Those of the French academy, made under greater pressures, gave a much smaller resistance; and the very experiments adduced in support of this theory are extremely deficient, wanting fully one-third of what the theory requires. The resistances by experiment were 15¼ and 26⅓, and the theory required 20½ and 39. The equation, however, deduced from the theory is greatly deficient in the expression of the pressures caused by the accumulation and depression, stating the heights of them as $= \frac{v^2}{2\phi}$.

They can never be so high, because the heaped-up water flows off at the sides, and it also comes in behind by the sides; so that the pressure is much less than half the weight of a column whose height is $\frac{v^2}{2g}$; both because the accumulation and depression are less at the sides than in the middle, and because, when the body is wholly immersed, the accumulation is greatly diminished. Indeed, in this case, the final equation does not include their effects, though as real in this case as when part of the body is above water.

Upon the whole, we are somewhat surprised that an author of D'Ulloa's eminence should have adopted a theory so unnecessarily and so improperly embarrassed with foreign circumstances, and that Mr Prony should have inserted it with the explanation by which he was to abide, in a work destined for practical use.*

This point, or the effect of deep immersion, is still much contested; and it is a received opinion, by many not accustomed to mathematical researches, that the resistance is greater in greater depths. This is assumed as an important principle by Mr Gordon, author of a *Theory of Naval Architecture*; but on very vague and slight grounds: and the author seems unacquainted with the manner of reasoning on such subjects. It shall be considered afterwards.

With these corrections it may be asserted that theory and experiment agree very well in this respect, and that resistance may be asserted to be in the duplicate ratio of the velocity.

We have been more minute on this subject, because it is the leading proposition in the theory of the action of fluids. Newton's demonstration of it takes no notice of the manner in which the various particles of the fluid are put in motion, or the motion which each in particular acquires. He only shews, that if there be nothing concerned in the communication but pure inertia, the sum-total of the motions of the particles, estimated in the direction of the bodies motion, or that of the stream, will be in the duplicate ratio of the velocity. It was therefore of importance to show that this part of the theory was just. To do this, we had to consider the effect of every circumstance which could be combined with the inertia of the fluid. All these had been foreseen by that great man, and are most briefly, though perspicuously, mentioned in the last scholium to prop. 36. B. II.

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* An examination of Dr Robison's objections to D'Ulloa's theory, by Mr Watts, will be found in the *Edinburgh Philosophical Journal*, vol. IV. p. 315.—Ed.

2. It appears, from a comparison of all the experiments, that the impulses and resistances are very nearly in the proportion of the surfaces. They appear, however, to increase somewhat faster than the surfaces. The Chevalier Borda found that the resistance, with the same velocity, to a surface of

9 inches	was	9	instead of	9
16		17,535		16
36		42,750		36
81		104,737		81

The deviation in these experiments from the theory increases with the surface, and is probably much greater in the extensive surfaces of the sails of ships and wind-mills, and the hulls of ships.

3. The resistances do by no means vary in the duplicate ratio of the sines of the angles of incidence.

As this is the most interesting circumstance, having a chief influence on all the particular modifications of the resistance of fluids; and as on this depends the whole theory of the construction and working of ships, and the action of water on our most important machines, and seems most immediately connected with the mechanism of fluids, it merits a very particular consideration. We cannot do a greater service than by rendering more generally known the excellent experiments of the French academy.

Fifteen boxes or vessels were constructed, which were two feet wide, two feet deep, and four feet long. One of them was a parallelopiped of these dimensions; the others had prows of a wedge-form, the angle ACB (Plate IX. Fig. 7 and 8.) varying by 12 degrees from 12° to 180°; so that the angle of incidence increased by 6° from one to another. These boxes were dragged across a very large basin of smooth water (in which they were immersed two feet) by means of a line passing over a wheel connected with a cylinder, from which the actuating weight was suspended. The motion became perfectly uniform after a very little way; and the time of passing over 96 French feet with this uniform motion was very carefully noted. The resistance was measured by the weight employed, after deducting a certain quantity (properly estimated) for friction, and for the accumulation of the water against the anterior surface. The results of the many experiments are given in the following table; where column 1st contains the angle of the prow, column 2d contains the resistance as given by the preceding theory, column 3d contains the resistance exhibited in the experiments, and column 4th contains the deviation of the experiment from the theory.

I.	II.	III.	IV.
180	10000	10000	0
168	9890	9893	+3
156	9568	9578	+10
144	9045	9084	+39
132	8346	8446	+100
120	7500	7710	+210
108	6545	6925	+380
96	5523	6148	+625
84	4478	5433	+955
72	3455	4800	+1345
60	2500	4404	+1904
48	1654	4240	+2586
36	955	4142	+3187
24	432	4063	+3631
12	109	3999	+3890

The resistance to 1 square foot, French measure, moving with the velocity of 2,56 feet per second, was very nearly 7,625 pounds French.

Reducing these to English measures, we have the surface = 1,1363 feet, the velocity of the motion equal to 2,7263 feet per second, and the resistance equal to 8,234 pounds avoirdupois. The weight of a column of fresh water of this base, and having for its height the fall necessary for communicating this velocity, is 8,264 pounds avoirdupois. The resistances to other velocities were accurately proportional to the squares of the velocities.

There is great diversity in the value which different authors have deduced for the absolute resistance of water from their experiments. In the value now given nothing is taken into account but the inertia of the water. The accumulation against the forepart of the box was carefully noted, and the statical pressure backwards, arising from this cause, was subtracted from the whole resistance to the drag. There had not been a sufficient variety of experiments for discovering the share which tenacity and friction produced; so that the number of pounds set down here may be considered as somewhat superior to the mere effects of the inertia of the water. We think, upon the whole, that it is the most accurate determination yet given of the resistance to a body in motion; but we shall afterwards see reason for believing, that the impulse of a running stream having the same velocity is somewhat greater; and this is the form in which most of the experiments have been made.

Also observe, that the resistance here given is that to a vessel two feet broad and deep, and four feet long. The resistance to a plane of two feet broad and deep would probably have exceeded this in the proportion of 15,22 to 14,54, for reasons we shall see afterwards.

From the experiments of Buat, it appears that a body of one foot square, French measure, and two feet long, having its centre 15 inches under water, moving three French feet per second, sustained a pressure of 14,54 French pounds, or 15,63 English. This reduced in the proportion of 3^2 to $2,56^2$ gives 11,43 pounds, considerably exceeding the 8,24.

Bouguer, in his *Manœuvre des Vaisseaux*, says, that he found the resistance of sea-water to a velocity of one foot to be 23 ounces *poids des Marc*.

Borda found the resistance of sea-water to the face of a cubic foot, moving against the water one foot per second, to be 21 ounces nearly. But this experiment is complicated: the wave was not deducted; and it was not a plane, but a cube.

D'Ulloa found the impulse of a stream of sea-water, running two feet per second on a foot square, to be 15½ pounds English measure. This greatly exceeds all the values given by others.

From these experiments we learn, in the first place, that the direct resistance to a motion of a plane surface through water, is very nearly equal to the weight of a column of water having that surface for its base, and for its height the fall producing the velocity of the motion. This is but one half of the resistance determined by the preceding theory. It agrees, however, very well with the best experiments made by other philosophers on bodies totally immersed or surrounded by the fluid; and sufficiently shews, that there must be some fallacy in the principles or reasoning by which this result of the theory is supposed to be deduced. We shall have occasion to return to this again.

But we see that the effects of the obliquity of incidence deviate enormously from the theory, and that this deviation increases rapidly as the acuteness of the prow increases. In the prow of 60° the deviation is nearly equal to the whole resistance pointed out by the theory, and in the prow of

12^0 it is nearly 40 times greater than the theoretical resistance.

The resistance of the prow of 90^0 should be one-half the resistance of the base. We have not such a prow; but the medium between the resistance of the prow of 96 and 84 is 5790, instead of 500.

These experiments are very conform to those of other authors on plane surfaces. Robins found the resistance of the air to a pyramid of 45^0, with its apex foremost, was to that of its base as 1000 to 1411, instead of one to two. Borda found the resistance of a cube, moving in water in the direction of the side, was to the oblique resistance, when it was moved in the direction of the diagonal, in the proportion of 5⅓ to 7; whereas it should have been that of $\sqrt{2}$ to 1, or of 10 to 7 nearly. He also found, that a wedge whose angle was 90^0, moving in air, gave for the proportion of the resistances of the edge and base 7281 : 10000, instead of 5000 : 10000. Also when the angle of the wedge was 60^0, the resistances of the edge and base were 52 and 100, instead of 25 and 100.

In short, in all the cases of oblique plane surfaces, the resistances were greater than those which are assigned by the theory. The theoretical law agrees tolerably with observation in large angles of incidence, that is, in incidences not differing very far from the perpendicular; but in more acute prows the resistances are more nearly proportional to the sines of incidence than to their squares.

The academicians deduced from these experiments an expression of the general value of the resistance, which corresponds tolerably well with observation. Thus let x be the complement of the half angle of the prow, and let P be the direct pressure or resistance, with an incidence of 90°, and p the effective oblique pressure: then $p = P \times \text{cosine}^2\, x + 3{,}153 \left(\frac{x^\circ}{6^\circ}\right)^{3{,}25}$. This gives for a prow of 12^0 an error in defect about $\frac{1}{100}$, and in larger angles it is much nearer the truth; and this is exact enough for any practice.

This is an abundantly simple formula: but if we introduce it in our calculations of the resistances of curvilineal prows, it renders them so complicated as to be almost useless; and what is worse, when the calculation is completed for a curvilineal prow, the resistance which results is found to differ widely from experiment. This shows, that the motion of the fluid is so modified by the action of the most prominent part of the prow, that its impulse on what succeeds is greatly affected, so that we are not allowed to consider the prow as composed of a number of parts, each of which is affected as if it were detached from all the rest.

As the very nature of naval architecture seems to require curvilineal forms, in order to give the necessary strength, it seemed of importance to examine more particularly the deviations of the resistances of such prows from the resistances assigned by the theory. The academicians, therefore, made vessels with prows of a cylindrical shape; one of these was a half cylinder, and the other was one-third of a cylinder, both having the same breadth, viz. two feet, the same depth, also two feet, and the same length, four feet. The resistance of the half cylinder was to the resistance of the perpendicular prow in the proportion of 13 to 25, instead of being as 13 to 19,5. Borda found nearly the same ratio of the resistances of the half cylinder, and its diametrical plane when moved in air. He also compared the resistances of two prisms or wedges, of the same breadth and height. The first had its sides plane, inclined to the base in angles of 60°; the second had its sides portions of cylinders, of which the planes were the chords, that is, their sections were arches of circles of 60°. Their resistances were as 133 to 100, instead of being as 133 to 220, as required by the theory; and as the resistance of the first was greater in proportion to that of the base than the theory allows, the resistance of the last was less.

Mr Robins found the resistance of a sphere moving in air to be to the resistance of its great circle as 1 to 2.27; whereas theory requires them to be as 1 to 2. He found, at the same time, that the absolute resistance was greater than the weight of a cylinder of air of the same diameter, and having the height necessary for acquiring the velocity. It was greater in the proportion of 49 to 40 nearly.

Borda found the resistance of the sphere moving in water to be to that of its great circle as 1000 to 2508, and it was one-ninth greater than the weight of the column of water whose height was that necessary for producing the velocity. He also found the resistance of air to the sphere was to its resistance to its great circle as 1 to 2.45.

It appears, on the whole, that the theory gives the resistance of oblique plane surfaces too small, and that of curved surfaces too great; and that it is quite unfit for ascertaining the modifications of resistance arising from the figure of the body. The most prominent part of the prow changes the action of the fluid on the succeeding parts, rendering it totally different from what it would be were that part detached from the rest, and exposed to the stream with the same obliquity. It is of no consequence, therefore, to deduce any formula from the valuable experiments of the French academy. The experiments themselves are of great importance, because they give us the impulses on plane surfaces with every obliquity. They therefore put it in our power to select the most proper obliquity in a thousand important cases. By appealing to them, we can tell what is the proper angle of the sail for producing the greatest impulse in the direction of the ship's course; or the best inclination of the sail of a wind-mill, or the best inclination of the float of a water-wheel, &c. &c. These deductions will be made in their proper places in the course of this work. We see also, that the deviation from the simple theory is not very considerable till the obliquity is great; and that, in the inclinations which other circumstances would induce us to give to the floats of water-wheels, the sails of wind-mills, and the like, the results of the theory are sufficiently agreeable to experiment, for rendering this theory of very great use in the construction of machines. Its great defect is in the impulsions on curved surfaces, which puts a stop to our improvement of the science of naval architecture, and the working of ships.

But it is not enough to detect the faults of the theory: we should try to amend it, or to substitute another. It is a pity that so much ingenuity should have been thrown away in the application of a theory so defective. Mathematicians were seduced, as has been already observed, by the opportunity which it gave for exercising their calculus, which was a new thing at the time of publishing this theory. Newton saw clearly the defects of it, and makes no use of any part of it in his subsequent discussions, and plainly has used it merely as an introduction, in order to give some general notions in a subject quite new, and to give a demonstration of one leading truth, viz. the proportionality of the impulsions to the squares of the velocities. While we profess the highest respect for the talents and labours of the great mathematicians who have followed Newton in this most difficult research, we cannot help being sorry that some of the greatest of them continued to attach themselves to a theory which he neglected, merely because it afforded an opportunity of displaying their profound knowledge of the new calculus, of which they were willing to ascribe the discovery to Leibnitz. It has been in a great measure owing to this, that we have been so late in discovering our ignorance of the subject. Newton had himself pointed out all the defects of this theory; and he set himself to work to discover another which should be more conformable to the nature of things, retaining only such deductions from the other as his great sagacity assured him would stand the test of experiment. Even in this he seems to have been mistaken by his followers. He retained the proportionality of the resistance to the square of the velocity. This they have endeavoured to demonstrate in a manner conformable to Newton's determination of the oblique impulses of fluids; and under the cover of the agreement of this proposition with experiment, they introduced into mechanics a mode of expression, and even of conception, which is inconsistent with all accurate notions on these subjects. Newton's proposition was, that the motions communicated to the fluid, and therefore the motions lost by the body, in equal times, were as the squares of the velocities; and he conceived these as proper measures of the resistances. It is a matter of experience, that the forces or pressures by which a body must be supported in opposition to the impulses of fluids, are in this very proportion. In determining the *proportion* of the direct and oblique resistances of plane surfaces, he considers the resistances to arise from mutual collisions of the surface and fluid, repeated at intervals of time too small to be perceived. But, in making this comparison, he has no occasion whatever to consider this *repetition;* and when he assigns the proportion between the resistance of a cone and of its base, he, in fact, assigns the proportion between two *simultaneous* and instantaneous impulses. But the mathematicians who followed him have considered this repetition as equivalent to an augmentation of the initial or first impulse; and in this way have attempted to demonstrate, that the resistances are as the squares of the velocities. When the velocity is double, each impulse is double, and the number in a given time is double; therefore, say they, the resistance, and the force which will withstand it, is quadruple; and observation confirms their deduction: yet nothing is more gratuitous and illogical. It is very true, that the resistance, conceived as Newton conceives it, the loss of motion sustained by a body moving in the fluid, is quadruple; but the instantaneous impulse, and the force which can withstand it, is, by all the laws of mechanics, only double. What is the force which can withstand a double impulse? Nothing but a double impulse. Nothing but impulse can be opposed to impulse; and it is a gross misconception to think of stating any kind of comparison between impulse and pressure. It is this which has given rise to much jargon and false reasoning about the force of percussion. This is stated as infinitely greater than any pressure, and as equivalent to a pressure infinitely repeated. It forced the abettors of these doctrines at last to deny the existence of all pressures whatever, and to assert, that all motion, and tendency to motion, was the result of impulse.

In consequence of the many objections to the comparison of pure pressure with pure percussion or impulse, John Bernoulli and others were obliged at last to assert, that there were no perfectly hard bodies in nature, nor could be, but that all bodies were elastic; and that in the communication of motion by percussion, the velocities of both bodies were *gradually* changed by their mutual elasticity acting during the finite but imperceptible time of the collision. This was, in fact, giving up the whole argument, and banishing percussion, while their aim was to get rid of pressure. For what is elasticity but a pressure? and how shall *it* be produced? To act in this instance, must it arise from a still smaller impulse? But this will require another elasticity, and so on without end.

These are all legitimate consequences of this attempt to state a comparison between percussion and pressure. Numberless experiments have been made to confirm the statement. But nothing affords so specious an argument as the experimented proportionality of the impulse of fluids to the square of the velocity. Here is every appearance of the accumulation of an infinity of minute impulses, in the known ratio of the velocity, each to each, producing pressures which are in the ratio of the squares of the velocities.

The pressures are observed; but the impulses or percussions, whose accumulation produces these pressures, are only supposed. The rare fluid, introduced by Newton for the purpose already mentioned, either does not exist in nature, or does not act in the manner we have said, the particles making their impulse, and then escaping through among the rest without affecting their motion. We cannot indeed say what may be the proportion between the diameter and the distance of the particles. The first may be incomparably smaller than the second, even in mercury, the densest fluid which we are familiarly acquainted with; but although they do not touch each other, they act nearly as if they did, in consequence of their mutual attractions and repulsions. We have seen air a thousand times rarer in some experiments than in others, and therefore the distance of the particles at least ten times greater than their diameters; and yet, in this rare state, it propagates all pressures or impulses made on any part of it to a great distance, almost in an instant. It cannot be, therefore, that fluids act on bodies by impulse. It is very possible to conceive a fluid advancing with a flat surface against the flat surface of a solid. The very first and superficial particles may make an impulse; and if they were annihilated, the next might do the same: and if the velocity were double, these impulses would be double, and would be withstood by a double force, and not a quadruple, as is observed: and this very circumstance, that a quadruple force is necessary, should have made us conclude that it was not to impulse that this force was opposed. The first particles having made their stroke, and not being annihilated, must escape laterally. In their escaping, they effectually prevent every farther impulse, because they come in the way of those filaments which would have struck the body. The whole process seems to be somewhat as follows:

When the flat surface of the fluid has come into contact with the plane surface AB (Plate IX. Fig. 6.) perpendicular to the direction DC of their motion, they must deflect to both sides equally, and in equal portions, because no reason can be assigned why more should go to either side. By this means the filament EF, which would have struck the surface in G, is deflected *before it arrives at* the surface, and describes a curved path EFIHK, continuing its rectilineal motion to I, where it is intercepted by a filament immediately adjoining to EF, on the side of the middle filament DC. The different particles of DC may be supposed to impinge in succession at C, and to be deflected at right angles; and, gliding along CB, to escape at B. Each filament in succession, outwards from DC, is deflected in its turn; and being hindered from even touching the surface CB, it glides off in a direction parallel to it; and thus EF is deflected in I, moves parallel to CB from I to H, and is again deflected at right angles, and describes HK parallel to DC. The same thing may be supposed to happen on the other side of DC.

And thus it would appear, that except two filaments immediately adjoining to the line DC, which besects the surface at right angles, no part of the fluid makes any impulse on the surface AB. All the other filaments are merely pressed against it by the lateral filaments without them, which they turn aside, and prevent from striking the surface.

In like manner, when the fluid strikes the edge of a prism or wedge ACB (Plate IX. Fig. 7.) it cannot be said that any real impulse is made. Nothing hinders us from supposing C a mathematical angle or indivisible point, not susceptible of any impulse, and serving merely to divide the stream. Each filament EF is effectually prevented from impinging at G in the line of its direction, and with the obliquity of incidence EGC, by the filaments between EF and DC, which glide along the surface CA; and it may be supposed to be deflected when it comes to the line CF which besects the angle DCA, and again deflected and rendered parallel to DC at I. The same thing happens on the other side of DC; and we cannot in this case assert that there is any impulse.

We now see plainly how the ordinary theory must be totally unfit for furnishing principles of naval architecture, even although a formula could be deduced from such a series of experiments as those of the French academy. Although we should know precisely the impulse of the fluid on a surface GL (Fig. 8.) of any obliquity, when it is alone, detached from all others, we cannot in the smallest degree tell what will be the action of *part* of a stream of fluid advancing towards it, with the same obliquity, when it is preceded by an adjoining surface CG, having a different inclination; for the fluid will not glide along GL in the same manner as if it made part of a more extensive surface having the same inclination. The previous deflections are extremely different in these two cases; and the previous deflections are the only changes which we can observe in the motions of the fluid, and the only causes of that pressure which we observe the body to sustain, and which we call the impulse on it. This theory must, therefore, be quite unfit for ascertaining the action on a curved surface, which may be considered as made up of an indefinite number of planes.

We now see how it happens that the action of fluids on solid bodies may and must be opposed by pressures, and may be compared with, and measured by, the pressure of gravity. We are not comparing forces of different kinds, percussions with pressures, but pressures with each other. Let us see whether this view of the subject will afford us any method of comparison or absolute measurement.

When a filament of fluid, that is, a row of corpuscles, are turned out of their course EF (Fig. 6.) and made to take another course IH, force is required to produce this change of direction. The filament is prevented from proceeding by other filaments which lie between it and the body, and which deflected it in the same manner as it were contained in a bended tube, and it will press on the concave filament next to it as it would press on the concave side of the tube. Suppose such a bended tube ABE (Fig. 9.) and that a ball A is projected along it with any velocity, and moves in it without any friction, it is demonstrated, in elementary mechanics, that the ball will move with undiminished velocity, and will press on every point, such as B, of the concave side of the tube, in a direction BF perpendicular to the plane CBD, which touches the tube in the point B. This pressure on the adjoining filament, on the concave side of its path, must be withstood by that filament which deflects it; and it must be propagated across that filament to the next, and thus augment the pressure upon that next filament already pressed by the deflection of the intermediate filament, and thus there is a pressure towards the middle filament, and towards the body, arising from the deflection of all the outer filaments; and their accumulated sum must be conceived as immediately exerted on the middle filaments and on the body, because a perfect fluid transmits every pressure undiminished.

The pressure BF is equivalent to the two BH, BG, one of which is perpendicular and the other parallel, to the direction of the original motion. By the first, (taken in any point of the curvilineal motion of any filament) the two halves of the stream are pressed together: and in the case of Fig 6. and 7., exactly balance each other. But the pressures, such as BG, must be ultimately withstood by the surface ACB; and it is by these accumulated pressures that the solid body is urged down the stream: and it is these accumulated pressures which we observe and measure in our experiments. We shall anticipate a little, and say that it is most easily demonstrated, that when a ball A (Fig. 9.) moves with undiminished velocity in a tube so incurvated that its axis at E is at right angles to its axis at A, the accumulated action of the pressures, such as

BG, taken for every point of the path, is precisely equal to the force which would produce or extinguish the original motion.

This being the case, it follows most obviously, that if the two motions of the filaments are such as we have described and represented by Fig. 6., the whole pressure in the direction of the stream, that is, the whole pressure which can be observed on the surface, is equal to the weight of a column of fluid having the surface for its base, and twice the fall productive of the velocity for its height, precisely as Newton deduced it from other considerations; and it seems to make no odds whether the fluid be elastic or unelastic, if the deflections and velocities are the same. Now it is a fact, that no difference in this respect can be observed in the actions of air and water; and this had always appeared a great defect in Newton's theory; but it was only a defect of the theory attributed to him. But it is also true, that the observed action is but one-half of what is just now deduced from this improved view of the subject. Whence arises this difference? The reason is this: We have given a very erroneous account of the motions of the filaments. A filament EF does not move as represented in Fig. 6. with two rectangular inflections at I and at H, and a path IH between them parallel to CB. The process of nature is more like what is represented in Fig. 10. *It is observed*, that at the anterior part of the body AB, there remains a quantity of fluid ADB, almost, if not altogether, stagnant, of a singular shape, having two curved concave sides A *a* D, B *b* D, along which the middle filaments glide. This fluid is very slowly changed. The late Sir Charles Knowles made many experiments for ascertaining the paths of the filaments of water. At a distance up the stream, he allowed small jets of a coloured fluid, which did not mix with water, to make part of the stream; and the experiments were made in troughs with sides and bottom of plate-glass. A small taper was placed at a considerable height above, by which the shadows of the coloured filaments were most distinctly projected on a white plane held below the trough, so that they were accurately drawn with a pencil.

The still water ADC lasted for a long while before it was renewed; and it seemed to be gradually wasted by abrasion, by the adhesion of the surrounding water, which gradually licked away the outer parts from D to A and B; and it seemed to renew itself in the direction CD, opposite to the motion of the stream. There was, however, a considerable intricacy and eddy in this motion. Some (seemingly superficial) water was continually, but slowly, flowing outward from the line DC, while other water was seen within and below it, coming inwards and going backwards.

The coloured lateral filaments were most constant in their form, while the body was the same, although the velocity was in some cases quadrupled. Any change which this produced seemed confined to the superficial filaments.

As the filaments were deflected, they were also constipated, that is, the curved parts of the filaments were nearer each other than the parallel straight filaments up the stream, and this constipation was more considerable as the prow was more obtuse, and the deflection greater.

The inner filaments were ultimately more deflected than those without them; that is, if a line be drawn touching the curve EFIH in the point H of contrary flexure, where the concavity begins to be on the side next the body, the angle HKC, contained between the axis and this tangent line, is so much the greater as the filament is nearer the axis.

When the body exposed to the stream was a box of upright sides, flat bottom, and angular prow, like a wedge, having its edge also upright, the filaments were not all deflected laterally, as theory would make us expect; but the filaments near the bottom were also deflected downwards as well as laterally, and glided along at some distance under the bottom, forming lines of double curvature.

The breadth of the stream that was deflected was much greater than that of the body; and the sensible deflection begun at a considerable distance up the stream, especially in the outer filaments.

Lastly, the form of the curves was greatly influenced by the proportion between the width of the trough and that of the body. The curvature was always less when the trough was very wide in proportion to the body.

Great varieties were also observed in the motion or velocity of the filaments. In general, the filaments increased in velocity outwards from the body to a certain small distance, which was nearly the same in all cases, and then diminished all the way outward. This was observed by inequalities in the colour of the filaments, by which one could be observed to outstrip another. The retardation of those next the body seemed to proceed from friction; and it was imagined that without this the velocity there would always have been greatest.

These observations give us considerable information respecting the mechanism of these motions, and the action of fluids upon solids. The pressure in the duplicate ratio of the velocities comes here again into view. We found, that although the velocities were very different, the curves were precisely the same. Now the observed pressures arise from the transverse forces by which each particle of a filament is retained in its curvilineal path; and we know that the force by which a body is retained in any curve is directly as the square of the velocity, and inversely as the radius of curvature. The curvature, therefore, remaining the same, the transverse forces, and consequently the pressure on the body, must be as the square of the velocity: and, on the other hand, we can see pretty clearly (indeed it is rigorously demonstrated by D'Alembert), that whatever be the velocities, the curves *will* be the same. For it is known in hydraulics, that it requires a fourfold or ninefold pressure to produce a double or triple velocity. And as all pressures are propagated through a perfect fluid without diminution, this fourfold pressure, while it produces a double velocity, preduces also fourfold transverse pressures, which will retain the particles, moving twice as fast, in the same curvilineal paths. And thus we see that the impulses, as they are called, and resistances of fluids, have a certain relation to the weight of a column of fluid, whose height is the height necessary for producing the velocity. How it happens that a plane surface, immersed in an extended fluid, sustains just half the pressure which it would have sustained had the motions been such as are sketched in Fig. 6th, is a matter of more curious and difficult investigation. But we see evidently that the pressure must be less than what is there assigned; for the stagnant water a-head of the body greatly diminishes the ultimate deflections of the filaments: And it may be demonstrated, that when the part BE of the canal, Fig. 9. is inclined to the part AB in an angle less than 90°, the pressures BG along the whole canal are as the versed sine of the ultimate angle of deflection, or the versed sine of the angle which the part BE makes with the part AB. Therefore, since the deflections resemble more the sketch given in Fig. 10, the accumulated sum of all these forces BG of Fig. 9. must be less than the similar sum corresponding to Fig. 6. that is, less than the weight of the column of fluid having twice the productive height for its height. How it is just one-half shall be our next inquiry.

And here we must return to the labours of Newton. After many beautiful observations on the nature and mechanism of continued fluids, he says, that the resistance which they occasion is but one-half of that occasioned by the rare fluid which had been the subject of his former proposition; "which truth," says he, "I shall endeavour to show."

He then enters into another, as novel and as difficult an investigation, viz. the laws of hydraulics, and endeavours to ascertain the motion of fluids through orifices when urged by pressures of any kind. He endeavours to ascertain the velocity with which a fluid escapes through a horizontal orifice in the bottom of a vessel, by the action of its weight, and the pressure which this vein of fluid will exert on a little circle which occupies part of the orifice. To obtain this, he employs a kind of approximation and trial, of which it would be extremely difficult to give an extract; and then, by increasing the diameter of the vessel and of the hole to infinity, he accommodates his reasoning to the case of a plane surface exposed to an indefinitely extended stream of fluid; and, lastly, giving to the little circular surface the motion which he had before ascribed to the fluid, he says, that the resistance to a plane surface moving through an unelastic continuous fluid, is equal to the weight of a column of the fluid whose weight is one-half of that necessary for acquiring the velocity; and he says, that the resistance of a globe is, in this case, the same with that of a cylinder of the same diameter. The resistance, therefore, of the cylinder or circle is four times less, and that of the globe is twice less than their resistances on a rare elastic medium.

But this determination, though founded on principles or assumptions, which are much nearer to the real state of things, is liable to great objections. It depends on his method for ascertaining the velocity of the issuing fluid; a method extremely ingenious, but defective. The cataract, which he supposes, cannot exist as he supposes, descending by the full action of gravity, and surrounded by a funnel of stagnant fluid. For, in such circumstances, there is nothing to balance the hydrostatical pressure of this surrounding fluid; because the whole pressure of this central cataract is employed in producing its own descent. In the next place, the pressure which he determines is beyond all doubt only half of what is observed on a plane surface in all our experiments. And, in the third place, it is repugnant to all our experience, that the resistance of a globe or of a pointed body is as great as that of its circular base. His reasons are by no means convincing. He supposes them placed in a tube or canal; and since they are supposed of the same diameter, and therefore leave equal spaces at their sides, he concludes, that because the water escapes by their sides with the same velocity, they will have the same resistance. But this is by no means a necessary consequence. Even if the water should be allowed to exert equal pressures on them, the pressures being perpendicular to their surfaces, and these surfaces being inclined to the axis, while in the case of the base of a cylinder it is in the direction of the axis, there must be a difference in the accumulated or compound pressure in the direction of the axis. He indeed says, that in the case of the cylinder or the circle obstructing the canal, a quantity of water remains stagnant on its upper surface, viz. all the water whose motion would not contribute to the most ready passage of the fluid between the cylinder and the sides of the canal or tube; and that this water may be considered as frozen. If this be the case, it is indifferent what is the form of the body that is covered with this mass of frozen or stagnant water. It may be a hemisphere or a cone; the resistance will be the same. But Newton by no means assigns, either with precision or with distinct evidence, the form and magnitude of this standing water, so as to give confidence in the results. He contents himself with saying, that it is that water whose motion is not necessary or cannot contribute to the most easy passage of the water.

There remain, therefore, many imperfections in this theory. But notwithstanding these defects, we cannot but admire the efforts and sagacity of this great philosopher, who, after having discovered so many sublime truths of a mechanical nature, ventured to trace out a path for the solution of a problem which no person had yet attempted to bring within the range of mathematical investigation. And this solution, though inaccurate, shines throughout with that inventive genius, and that fertility of resource, which no man ever possessed in so eminent a degree.

Those who have attacked the solution of Sir Isaac Newton have not been more successful. Most of them, instead of principles, have given a great deal of calculus; and the chief merit which any of them can claim, is that of having deduced some single proposition which happens to quadrate with some single case of experiment, while their general theories are either inapplicable, from difficulty and obscurity, or are discordant with more general observation.

We must, however, except from this number Daniel Bernoulli, who was not only a great geometer, but one of the first philosophers of the age. He possessed all the talents, and was free from the faults of that celebrated family; and while he was the mathematician of Europe who penetrated farthest in the investigation of this great problem, he was the only person who felt, or at least who acknowledged, its great difficulty.

In the 2d volume of the *Comment. Petropol.* 1727, he proposes a formula for the resistance of fluids, deduced from considerations quite different from those on which Newton founded his solution. But he delivers it with modest diffidence, because he found that it gave a resistance four times greater than experiment. In the same dissertation he determines the resistance of a sphere to be one-half of that of its great circle. But in his subsequent theory of Hydrodynamics, he calls this determination in question. It is indeed founded on the same hypothetical principles which have been unskilfully detached from the rest of Newton's physics, and made the ground-work of all the subsequent theories on this subject.

In 1741, Bernoulli published another dissertation (in the 8th volume of the *Com. Petropol.*) on the action and resistance of fluids, limited to a very particular case; namely, to the impulse of a vein of fluid falling perpendicularly on an infinitely extended plane surface. This he demonstrates to be equal to the weight of a column of the fluid whose base is the area of the vein, and whose height is twice the fall producing the velocity. This demonstration is drawn from the true principles of mechanics and the acknowledged laws of hydraulics, and may be received as a strict physical demonstration. As it is the only proposition in the whole theory that has as yet received a demonstration accessible to readers not versant in all the refinement of modern analysis; and as the principles on which it proceeds will undoubtedly lead to a solution of every problem which can be proposed, once that our mathematical knowledge shall enable us to apply them—we think it our duty to give it in this place, although we must acknowledge, that this problem is so very limited, that it will hardly bear an application to any case that differs but a little from the express conditions of the problem. There do occur cases, however, in practice, where it may be applied to very great advantage.

Daniel Bernoulli gives two demonstrations; one of which may be called a popular one, and the other is more scientific and introductory to further investigation. We shall give both.

Bernoulli first determines the whole action exerted in the efflux of the vein of fluid. Suppose the velocity of efflux v is that which would be acquired by falling through the height h. It is well known that a body moving during the time of this fall with the velocity v would describe a space $2h$. The effect, therefore, of the hydraulic action is, that in the time t of the fall h, there issues a cylinder or prism of water whose base is the cross section s or area of the vein, and whose length is $2h$. And this quantity of matter is now moving with the velocity v. The quantity of motion, therefore, which is thus produced is $2shv$; and this quantity of motion is produced in the time t. And this is the accumulated effect of all the expelling forces, estimated in the direction of the efflux. Now, to compare this with the exertion of some pressing power with which we are familiarly acquainted, let us suppose this pillar $2\,s\,h$ to be frozen, and, being held in the hand, to be dropped. It is well known, that in the time t it will fall through the height h, and will acquire the velocity v, and now possesses the quantity of motion $2\,s\,h\,v$—and all this is the effect of its weight. The weight, therefore, of the pillar $2\,s\,h$ produces the same effect, and in the same time, and (as may easily be seen) in the same gradual manner, with the expelling forces of the fluid in the vessel, which expelling forces arise from the pressure of all the fluid in the vessel. Therefore the accumulated hydraulic pressure, by which a vein of a heavy fluid is forced out through an orifice in the bottom or side of a vessel, is equal (when estimated in the direction of the efflux) to the weight of a column of the fluid, having for its base the section of the vein, and twice the fall productive of the velocity of efflux for its height.

Now let ABDC (Fig. 11.) be a quadrangular vessel with upright plane sides, in one of which is an orifice EF. From every point of the circumference of this orifice, suppose horizontal lines E e, F f, &c. which will mark a similar surface on the opposite side of the vessel. Suppose the orifice EF to the shut. There can be no doubt but that the surfaces EF and $e\,f$ will be equally pressed in opposite directions. Now open the orifice EF; the water will rush out, and the pressure on EF is now removed. There will therefore be a tendency in the vessel to move back in the direction E e. And this tendency must be precisely equal and opposite to the whole effort of the expelling forces.

Now, let this stream of water be received on a circular plane MN, perpendicular to its axis, and let this circular plane be of such extent, that the vein escapes from its sides in an infinitely thin sheet, the water flowing off in a direction parallel to the plane. The vein by this means will expand into a trumpet-like shape, having curved sides, EKG, FLH, Fig. 12. We abstract at present the action of gravity which would cause the vein to bend downwards, and occasion a greater velocity at H than at G; and we suppose the velocity equal in every point of the circumference. It is plain, that if the action of gravity be neglected after the water has issued through the orifice EF, the velocity in every point of the circumference of the plane MN will be that of the efflux through EF.

Now, because EKG is the natural shape assumed by the vein, it is plain, that if the whole vein were covered by a tube or mouth-piece, fitted to its shape, and perfectly polished, so that the water shall glide along it, without any friction (a thing which we may always suppose), the water will exert no pressure whatever on this trumpet mouthpiece. Lastly, let us suppose that the plane MN is attached to the mouth-piece by some bits of wire, so as to allow the water to escape all round by the narrow chink between the mouth-piece and the plane: We have now a vessel consisting of the upright part ABDC, the trumpet GKEFLH, and the plane MN; and the water is escaping from every point of the circumference of the chink GHNM with the velocity v. If any part of this chink were shut up, there would be a pressure on that part equivalent to the force of efflux from the opposite part. Therefore, when all is open, these efforts of efflux balance each other all round. There is not therefore any tendency in this compound vessel to move to any side. But take away the plane MN, and there would immediately arise a pressure in the direction Ee equal to the weight of the column $2sh$. This is therefore balanced by the pressure on the circular plane MN, which is therefore equal to this weight, and the proposition is demonstrated.

A number of experiments were made by Kraft at St Petersburg, by receiving the vein on a plane MN (Fig. 11.) which was fastened to the arm of a balance OPQ, having a scale R hanging on the opposite arm. The resistance or pressure on the plane was measured by weights put into the scale R; and the velocity of the jet was measured by means of the distance KH, to which it spouted on a horizontal plane.

The results of these experiments were as conformable to the theory as could be wished. The resistance was always a little less than what the theory required, but greatly exceeded its half; the result of the generally received theories. This defect should be expected; for the demonstration supposes the plane MN to be infinitely extended, so that the film of water which issues through the chink may be accurately parallel to the plane. This never can be completely effected. Also it was supposed, that the velocity was justly measured by the amplitude of the parabola EGK. But it is well known that the very putting the plane MN in the way of the jet, though at the distance of an inch from the orifice, will diminish the velocity of the efflux through this orifice. This is easily verified by experiment. Observe the time in which the vessel will be emptied when there is no plane in the way. Repeat the experiment with the plane in its place; and more time will be necessary. The following is a note of a course of experiments, taken as they stand, without any selection.

	N° 1	2	3	4	5	6
Resist. by theory	1701	1720	1651	1602	1528	1072
Resist. by expt.	1403	1463	1486	1401	1403	1021
Difference	298	257	165	201	125	51

In order to demonstrate this proposition in such a manner as to furnish the means of investigating the whole mechanism and action of moving fluids, it is necessary to premise an elementary theorem of curvilineal motions.

If a particle of matter describes a curve line ABCE

(Fig. 13.) by the continual action of deflecting forces, which vary in any manner, both with respect to intensity and direction, and if the action of these forces, in every point of the curve, be resolved into two directions, perpendicular and parallel to the initial direction AK; then,

1. The accumulated effect of the deflecting forces, estimated in a direction AD perpendicular to AK, is to the final quantity of motion as the sine of the final change of direction is to radius.

Let us first suppose that the accelerating forces act by starts, at equal intervals of time, when the body is in the points A, B, C, E. And let AN be the deflecting force, which, acting at A, changes the original direction AK to AB. Produe AB till BH=AB, and complete the parallelogram BFCH. Then FB is the force which, by acting at B, changed the motion BH (the continuation of AB) to BC. In like manner make C*h* (in BC produced) equal to BC, and complete the parallelogram C*f*E*h*. C*f* is the deflecting force at C, &c. Draw BO parallel to AN, and GBK perpendicular to AK. Also draw lines through C and E perpendicular to AK, and draw through B and C lines parallel to AK. Draw also HL, *h l* perpendicular, and FG, HI, *h i*, parallel to AK.

It is plain that BK is BO or AN estimated in the direction perpendicular to AK, and that BG is BF estimated in the same way. And since BH=AB, HL or IM is equal to BK. Also CI is equal to BG. Therefore CM is equal to AP+BG. By similar reasoning it appears that $\mathrm{E}\,m = \mathrm{E}\,i + h\,l, = \mathrm{C}\,g + \mathrm{CM}, = \mathrm{C}\,g + \mathrm{BG} + \mathrm{AP}$.

Therefore if CE be taken for the measure of the final velocity or quantity of motion, E *m* will be the accumulated effect of the deflecting forces estimated in the direction AD perpendicular to AK. But E *m* is to CE as the sine of *m* CE is to radius; and the angle *m* CE is the angle contained between the initial and final directions, because C *m* is parallel to AK. Now let the intervals of time diminish continually and the frequency of the impulses increase. The deflection becomes ultimately continuous, and the motion curvilineal, and the proposition is demonstrated.

We see that the initial velocity and its subsequent changes do not affect the conclusion, which depends entirely on the final quantity of motion.

2. The accumulated effect of the accelerating forces, when estimated in the direction AK of the original motion, or in the opposite direction, is equal to the difference between the initial quantity of motion, and the product of the final quantity of motion by the cosine of the change of direction.

$$\text{For } \mathrm{C}m = \mathrm{C}\,l - m\,l, = \mathrm{BM} - fq$$
$$\mathrm{BM} = \mathrm{BL} - \mathrm{ML}, = \mathrm{AK} - \mathrm{FG}$$
$$\mathrm{AK} = \mathrm{AO} - \mathrm{OK}, = \mathrm{AO} - \mathrm{PN}.$$

Therefore $\mathrm{PN} + \mathrm{FG} + f\mathrm{Q}$ (the accumulated impulse in the direction OA) $= \mathrm{AO} - \mathrm{CM}, = \mathrm{AO} - \mathrm{CE} \times$ cosine of ECM.

Cor. 1. The same action, in the direction opposite to that of the original motion, is necessary for causing a body to move at right angles to its former direction as for stopping its motion. For in this case, the cosine of the change of direction is $= o$, and $\mathrm{AO} - \mathrm{CE} \times \text{cosine ECM} = \mathrm{AO} - o$, $= \mathrm{AO}$, $=$ the original motion.

Cor. 2. If the initial and final velocities are the same, the accumulated action of the accelerating forces, estimated in the direction OA, is equal to the product of the original quantity of motion by the versed sine of the change of direction.

The application of these theorems, particularly the second, to our present purpose is very obvious. All the filaments of the jet were originally moving in the direction of its axis, and they are finally moving along the resisting plane, or perpendicular to their former motion. Therefore their transverse forces in the direction of the axis are (*in cumulo*) equal to the force which would stop the motion. For the aggregate of the simultaneous forces of every particle in the whole filament is the same with that of the successive forces of one particle, as it arrives at different points of its curvilineal path. All the transverse forces, estimated in a direction perpendicular to the axis of the vein, precisely balance and sustain each other; and the only forces which can produce a sensible effect are those in a direction parallel to the axis. By these all the inner filaments are pressed towards the plane MN, and must be withstood by it. It is highly probable, nay certain, that there is a quantity of stagnant water in the middle of the vein which sustains the pressures of the moving filaments without it, and transmits it to the solid plane. But this does not alter the case. And, fortunately, it is of no consequence what changes happen in the velocities of the particles while each is describing its own curve. And it is from this circumstance, peculiar to this particular case of perpendicular impulse, that we are able to draw the conclusion. It is by no means difficult to demonstrate that the velocity of the external surface of this jet is constant, and indeed of every jet which is not acted on by external forces after it has quitted the orifice: but this discussion is quite unnecessary here. It is however extremely difficult to ascertain, even in this most simple case, what is the velocity of the internal filaments in the different points of their progress.

Such is the demonstration which Bernoulli has given of this proposition. Limited as it is, it is highly valuable, because derived from the true principles of hydraulics.

He hoped to render it more extensive and applicable to oblique impulses, when the axis AC of the vein (Fig. 13. nº 2.) is inclined to the plane in an angle ACN. But here all the simplicity of the case is gone, and we are now obliged to ascertain the motion of each filament. It might not perhaps be impossible to determine what must happen in the plane of the figure, that is, in a plane passing through the axis of the vein, and perpendicular to the plane MN. But even in this case it would be extremely difficult to determine how much of the fluid will go in the direction EKG, and what will go in the path FLH, and to ascertain the form of each filament, and the velocity in its different points. But in the real state of the case, the water will dissipate from the centre C on every side; and we cannot tell in what proportions. Let us however consider a little what happens in the plane of the figure, and suppose that all the water goes either in the course EKG or in the course FLH. Let the quantities of water which take these two courses have the proportions of p and π. Let $\sqrt{2a}$ be the velocity at A, $\sqrt{2b}$ be the velocity at G, and $\sqrt{2\beta}$ be the velocity at H. ACG and ACH are the two changes of direction, of which let c and $-c$ be the cosines. Then, adopting the former reasoning, we have the pressure of the watery plate GKEACM on the plane in the direction AC $= \frac{p}{p+\pi}\,\overline{+2a-2cb}$, and the pressure of the plate HLFACN $= \frac{\pi}{p+\pi} \times \overline{2a+2c\beta}$, and their sum $= \frac{p \times \overline{2a-2cb} + \pi \times \overline{2a+2c\beta}}{p+\pi}$; which being multiplied by the sine of ACM or $\sqrt{1-c^2}$, gives the pressure perpendicular to the plane MN $= \frac{p \times \overline{2a-2cb} + \pi \times \overline{2a+2c\beta}}{p+\pi} \sqrt{1-c^2}$.

But there remains a pressure in the direction perpendicular to the axis of the vein, which is not balanced, as in the former case, by the equality on opposite sides of the axis. The pressure arising from the water which escapes at G has an effect opposite to that produced by the water which escapes at H. When this is taken into account, we shall find that their joint efforts perpendicular to AC are $\frac{p-\pi}{p+\pi} \times 2a\sqrt{1-c^2}$, which, being multiplied by the cosine of ACM, gives the action perpendicular to MN $=\frac{p-\pi}{p+\pi}\times 2ac\sqrt{1-c^2}$,

The sum or joint effort of all these pressures is

$$\frac{p\times\overline{2a-2cb}+\pi\times\overline{2a+2c\beta}}{p+\pi}\sqrt{1-c^2}+\frac{p-\pi}{p+\pi}\times 2ac\sqrt{1-c^2}.$$

Thus, from this case, which is much simpler than can happen in nature, seeing that there will always be a lateral efflux, the determination of the impulse is as uncertain and vague as it was sure and precise in the former case.

It is therefore without proper authority that the absolute impulse of a vein of fluid on a plane which receives it wholly, is asserted to be proportional to the sine of incidence. If indeed we suppose the velocity in G and H are equal to that at A, then $b=\beta,=a$, and the whole impulse is $2a\sqrt{1-c^2}$, as is commonly supposed. But this cannot be. Both the velocity and quantity at H are less than those at G. Nay, frequently there is no efflux on the side H when the obliquity is very great. We may conclude in general, that the oblique impulse will always bear to the direct impulse a greater proportion than that of the sine of incidence to radius. If the whole water escapes at G, and none goes off laterally, the pressure will be $\overline{2a+2ac-2bc}\times\sqrt{1-c^2}$. The experiments of the Abbé Bossut show in the plainest manner that the pressure of a vein, striking obliquely on a plane which receives it wholly, diminishes faster than in the ratio of the square of the sine of incidence; whereas, when the oblique plane is wholly immersed in the stream, the impulse is much greater than in this proportion, and in great obliquities is nearly as the sine.

Nor will this proposition determine the impulse of a fluid on a plane wholly immersed in it, even when the impulse is perpendicular to the plane. The circumstance is now wanting on which we can establish a calculation, namely, the angle of final deflection. Could this be ascertained for each filament, and the velocity of the filament, the principles are completely adequate to an accurate solution of the problem. In the experiments which we mentioned to have been made under the inspection of Sir Charles Knowles, a cylinder of six inches diameter was exposed to the action of a stream moving precisely one foot per second; and when certain deductions were made for the water which was held adhering to the posterior base (as will be noticed afterward), the impulse was found equal to $3\frac{1}{8}$ ounces avoirdupois. There were 36 coloured filaments distributed on the stream, in such situations as to give the most useful indications of their curvature. It was found necessary to have some which passed under the body and some above it; for the form of these filaments, at the same distance from the axis of the cylinder, was considerably different; and those filaments which were situated in planes neither horizontal nor vertical took a double curvature. In short, the curves were all traced with great care, and the deflecting forces were computed for each, and reduced to the direction of the axis; and they were summed up in such a manner as to give the impulse of the whole stream. The deflections were marked as far a-head of the cylinder as they could be assuredly observed. By this method the impulse was computed to be $2\frac{15}{16}$ ounces, differing from observation $\frac{3}{16}$ of an ounce, or about $\frac{1}{16}$ of the whole; a difference which may most reasonably be ascribed to the adhesion of the water, which must be most sensible in such small velocities. These experiments may therefore be considered as giving all the confirmation that can be desired of the justness of the principles. This indeed hardly admits of a doubt; but, alas! it gives us but small assistance; for all this is empirical, in as far as it leaves us in every case the task of observing the form of the curves and the velocities in their different points. To derive service from this most judicious method of Daniel Bernoulli, we must discover some method of determining, *a priori*, what will be the motion of the fluid, whose course is obstructed by a body of any form. And here we cannot omit taking notice of the casual observation of Sir Isaac Newton when attempting to determine the resistance of the plane surface or cylinder, or sphere exposed to a stream moving in a canal. He says that the form of the resisting surface is of less consequence, because there is always a quantity of water stagnant upon it, and which may therefore be considered as frozen; and he therefore considers that water only whose motion is necessary for the most expeditious discharge of the water in the vessel. He endeavours to discriminate that water from the rest; and although it must be acknowledged that the principle which he assumes for this purpose is very gratuitous, because it only shows that *if certain portions of the water*, which he determines very ingeniously, were really frozen, the rest will issue as he says, and will exert the pressure which he assigns; still we must admire his fertility of resource, and his sagacity in thus foreseeing what subsequent observation has completely confirmed. We are even disposed to think, that in this casual observation Sir Isaac Newton has pointed out the only method of arriving at a solution of the problem; and that if we could discover *what motions are not necessary for the most expeditious passage of the water*, and could thus determine the form and magnitude of the stagnant water which adheres to the body, we should much more easily ascertain the real motions which occasion the observed resistance.

The Chevalier D'Arcy has shewn, that in the trains of natural operations which terminate in the production of motion in a particular direction, the intermediate communications of motion are such that the smallest possible quantity of motion is produced. We seem obliged to conclude, that this law will be observed in the present instance; and it seems a problem not above our reach to determine the motions which result from it. We would recommend the problem to the eminent mathematicians in some simple case, such as the proposition already demonstrated by Daniel Bernoulli, or the perpendicular impulse on a cylinder included in a tubular canal; and if they succeed in this, great things may be expected. We think that experience gives great encouragement. We see that the resistance to a plane surface is a little greater than the weight of a column of the fluid having the fall productive of the velocity for its height, and the small excess is most probably owing to adhesion, and the measure of the real resistance is probably precisely this weight. The velocity of a spouting fluid was found, in fact, to be that acquired by falling from the surface of the fluid; and it was by looking at this, as at a pole star, that Newton, Bernoulli, and others, have with great sagacity and ingenuity discovered much of the laws of hydraulics, by searching for principles which would give this result. We may hope for similar success.

In the mean time, we may receive this as a physical truth, that the perpendicular impulse or resistance of a plane surface, wholly immersed in the fluid, is equal to the weight of the column having the surface for its base, and the fall producing the velocity for its height.

This is the medium result of all experiments made in these precise circumstances. And it is confirmed by a set of experiments of a kind wholly different, and which seem to point it out more certainly as an immediate consequence of hydraulic principles.

If Pitot's tube be exposed to a stream of fluid issuing from a reservoir or vessel, as represented in Fig. 14. with the open mouth I pointed directly against the stream, the fluid is observed to stand at K in the upright tube, precisely on a level with the fluid AB in the reservoir. Here is a most unexceptionable experiment, in which the impulse of the stream is actually opposed to the hydrostatical pressure of the fluid on the tube. Pressure is in this case opposed to pressure, because the issuing fluid is deflected by what stays in the mouth of the tube, in the same way in which it would be deflected by a firm surface. We shall have occasion by and by to mention some most valuable and instructive experiments made with this tube.

It was this which suggested to Euler another theory of the impulse and resistance of fluids, which must not be omitted, as it is applied in his elaborate performance on the Theory of the Construction and Working of Ships, in two volumes 4to. He supposes a stream of fluid ABCD (Fig. 15.), moving with any velocity, to strike the plane BD perpendicularly, and that part of it goes through a hole EF, forming a jet EGHF. Euler says, that the velocity of this jet will be the same with the velocity of the stream. Now compare this with an equal stream issuing from a hole in the side of a vessel with the same velocity. The one stream is urged out by the pressure occasioned by the impulse of the fluid; the other is urged out by the impulse of the fluid; the other is urged out by the pressure of gravity. The effects are equal, and the modifying circumstances are the same. The causes are therefore equal, and the pressure occasioned by the impulse of a stream of fluid, moving with any velocity, is equal to the weight of a column of fluid whose height is productive of this velocity, &c. He then determines the oblique impulse by the resolution of motion, and deduces the common rules of resistance, &c.

Not a shadow of argument is given for the leading principle in this theory, viz. that the velocity of the jet is the same with the velocity of the stream. None can be given, but saying, that the pressure is equivalent to its production; and this is assuming the very thing he labours to prove. The matter of fact is, that the velocity of the jet is greater than that of the stream, and may be greater almost in any proportion. Which curious circumstance was discovered and ingeniously explained long ago by Daniel Bernoulli in his *Hydrodynamica*. It is evident that the velocity must be greater. Were a stream of sand to come against the plane, what goes through would indeed preserve its velocity unchanged: but when a real fluid strikes the plane, all that does not pass through is deflected on all sides; and by these deflections forces are excited, by which the filaments which surround the cylinder immediately fronting the hole are made to press the cylinder on all sides, and as it were squeeze it between them: and thus the particles at the hole must of necessity be accelerated, and the velocity of the jet must be greater than that of the stream. We are disposed to think that, in a fluid perfectly incompressible, the velocity will be double, or at least increased in the proportion of 1 to $\sqrt{2}$. If the fluid is in the smallest degree compressible, even in the very small degree that water is, the velocity at the first impulse may be much greater. D. Bernoulli found, that a column of water moving 5 feet per second, in a tube some hundred feet long, produced a velocity of 186 feet per second in the first moment.

There being this radical defect in the theory of Euler, it is needless to take notice of its total insufficiency for explaining oblique impulses and the resistance of curvilineal prows.

M. d'Alembert has attempted a solution of that problem in a method entirely new and extremely ingenious. He saw clearly, that all the followers of Newton had forsaken the path which he had marked out for them in the second part of his investigation, and had merely amused themselves with the mathematical discussion with which his introductory hypothesis gave them an opportunity of occupying themselves. He paid the deserved tribute of applause to Daniel Bernoulli for having introduced the notion of pure pressure as the chief agent in this business; and he saw that he was in the right road, and that it was from hydrostatical principles alone that we had any chance of explaining the phenomena of hydraulics. Bernoulli had only considered the pressures which were excited in consequence of the curvilineal motions of the particles. M. d'Alembert even thought that these pressures were not the consequences, but the causes, of these curvilineal motions. No internal motion can happen in a fluid but in consequence of an unbalanced pressure; and every such motion will produce an inequality of pressure, which will determine the succeeding motions. He therefore endeavoured to reduce all to the discovery of those disturbing pressures, and thus to the laws of hydrostatics. He had long before this hit on a very refined and ingenious view of the action of bodies on each other, which had enabled him to solve many of the most difficult problems concerning the motions of bodies, such as the centre of oscillation, of spontaneous conversion, the precession of the equinoxes, &c. &c. with great facility and elegance. He saw that the same principle would apply to the action of fluid bodies. The principle is this:

"*In whatever manner any number of bodies are supposed to act on each other, and by these actions come to change their present motions, if we conceive that the motion which each body would have in the following instant (if it became free), is resolved into two other motions; one of which is the motion which it really takes in the following instant; the other will be such, that if each body had no other motion but this second, the whole bodies would have remained in equilibrio.*" We here observe, that "the motion which each body would have in the following instant, if it became free," is a continuation of the motion which it has in the first instant. It may therefore perhaps be better expressed thus:

If the motions of bodies, any how acting on each other, be considered in two consecutive instants, and if we conceive the motion which it has in the first instant as compounded of two others, one of which is the motion which it actually takes in the second instant, the other is such, that *if each body had only those second motions, the whole system would have remained in equilibrio.*

The proposition itself is evident. For if these second motions be not such as that an equilibrium of the whole system would result from them, the other component motions would not be those which the bodies really have after the change; for they would necessarily be altered by these unbalanced motions. See D'Alembert *Essai de Dynamique.*

Assisted by this incontestable principle, M. d'Alembert demonstrates, in a manner equally new and simple, those propositions which Newton had so cautiously deduced from his hypothetical fluid, shewing that they were not limited to this hypothesis, viz. that the motions produced by similar bodies, similarly projected in them, would be similar; that whatever were the pressures, the curves described by the particles would be the same; and that the resistances would be proportional to the squares of the velocities. He then comes to consider the fluid as having its motions constrained by the form of the canal or by solid obstacles interposed.

It is evident, that if the body ADCE (Fig. 16.) did not form an obstruction to the motion of the water, the particles would describe parallel lines TF, OK, PS, &c. But while yet at a distance from the body in F, K, S, they gradually change their directions, and describe the curves FM, K *m*, S *n*, so much more incurvated as they are nearer to the body. At a certain distance ZY this curvature will be insensible, and the fluid included in the space ZYHQ will move uniformly as if the solid body were not there. The motions on the other side of the axis AC will be the same; and we need only attend to one half, and we shall consider these as in a state of permanency.

No body changes either its direction or velocity otherwise than by insensible degrees: therefore the particle which is moving in the axis will not reach the vertex A of the body, where it behoved to deflect instantaneously at right angles. It will therefore begin to be deflected at some point F a-head of the body, and will describe a curve FM, touching the axis in F, and the body in M; and then, gliding along the body, will quit it at some point L, describing a tangent curve, which will join the axis again (touching it) in R; and thus there will be a quantity of stagnant water FAM before or a-head of the body, and another LCR behind or astern of it.

Let a be the velocity of a particle of the fluid in any instant, and a' its velocity in the next instant. The velocity a may be considered as compounded of a' and a''. If the particles tended to move with the velocities a'' only, the whole fluid would be in equilibrio (general principle), and the pressure of the fluid would be the same as if all were stagnant, and each particle were urged by a force $\frac{a''}{\dot{t}}$, $\dot{t}$ expressing an indefinitely small moment of time. (N.B. $\frac{a''}{\dot{t}}$, is the proper expression of the accelerating force, which, by acting during the moment $\dot{t}$, would generate the velocity a''; and a'' is supposed an indeterminate quantity, different perhaps for each particle). Now, let a be supposed constant, or $a=a'$. In this case $a''=o$. That is to say, no pressure whatever will be exerted on the solid body unless there happen changes in the velocities or directions of the particles.

Let a and a' then be the motions of the particles in two consecutive instants. They would be in equilibrio if urged only by the forces $\frac{a''}{\dot{t}}$. Therefore if γ be the point where the particles which describe the curve FM begin to change their velocity, the pressure in D would be equal to the pressure which the fluid contained in the canal γ FMD would exert, if each particle were solicited by its force $\frac{a}{\dot{t}}$. The question is therefore reduced to the finding the curvature in the canal γ FMD, and the accelerating forces $\frac{a''}{\dot{t}}$ in its different parts.

It appears, in the first place, that no pressure is exerted by any of the particles along the curve FM: for suppose that the particle *a* (Fig. 18.) describes the definitely small straight line *a b* in the first instant, and *b c* in the second instant; produce *a b* till $b\,d = a\,b$, and joining *d c*, the motion *a b* or *b d* may be considered as composed of *b c*, which the particle really takes in the next instant, and a motion *d c* which should be destroyed. Draw *b i* parallel to *d c*, and *i e* perpendicular to *b c*. It is plain that the particle *b*, solicited by the forces *b e*, *e i* (equivalent to *d c*) should be in equilibrio. This being established, *b e* must be $= o$, that is, there will be no accelerating or retarding force at *b*; for if there be, draw *b m* perpendicular to *b* F, and the parallel *n q* infinitely near it. The part *b n* of the fluid contained in the canal *b n q m* would sustain some pressure from *b* towards *n*, or from *n* towards *b*. Therefore, since the fluid in this stagnant canal should be in equilibrio, there must also be some action, at least in one of the parts *b m*, *m q*, *q n*, to counterbalance the action on the part *b n*. But the fluid is stagnant in the space FAM (in consequence of the law of continuity). Therefore there is no force which can act on *b m*, *m q*, *q n*; and the pressure in the canal in the direction *b n* or *n b* is nothing, (Fig. 17.) or the force $b\,e = o$, and the force *i e* is perpendicular to the canal; and there is therefore no pressure in the canal FM, except what proceeds from the part γ F, or from the force *e i*; which last being perpendicular to the canal, there can be no force exerted on the point M, but what is propagated from the part γ F.

The velocity therefore in the canal FM is constant if finite, or infinitely small if variable: for, in the first case, the force $b\,e$ would be absolutely nothing; and in the second case it would be an infinitesimal of the second order, and may be considered as nothing in comparison with the velocity, which is of the first order. We shall see by and by that the last is the real state of the case. Therefore the fluid, before it begins to change its direction in F, begins to change its velocity in some point γ a-head of F, and by the time that it reaches F its velocity is as it were annihilated.

Cor. 1. Therefore the pressure in any point D (Fig 16.) arises both from the retardations in the part γ F, and from the particles which are in the canal MD: as these last move along the surface of the body, the force, $\frac{a''}{\dot{t}}$, destroyed in every particle, is compounded of two others, one in the direction of the surface, and the other perpendicular to it; call these p and p'. The point D is pressed perpendicularly to the surface MD; 1st, by all the forces p in the curve MD; 2d, by the force p' acting on the single point D. This may be neglected in comparison of the indefinite number of the others: therefore taking in the arch MD, an infinitely small portion N$m = s$, the pressure on D, perpendicular to the surface of the body, will be $=\int p\,\dot{s}$; and this fluent must be so taken as to be $= o$ in the point M.

Cor. 2. Therefore, to find the pressure on D, we must find the force p on any point N. Let u be the velocity of the particle N, in the direction Nm in any instant, and $u+\dot{u}$ its velocity in the following instant; we must have $p=\frac{-\dot{u}}{\dot{t}}$. Therefore the whole question is reduced to finding the velocity u in every point N, in the direction Nm.

And this is the aim of a series of propositions which follow, in which the author displays the most accurate and precise conception of the subject, and great address and elegance in his mathematical analysis. He at length brings out an equation which expresses the pressure on the body in the most general and unexceptionable manner; but we must say; d'Alembert has not been able to exemplify the application of the equation to the simplest case which can be proposed, such as the direct impulse on a plane surface wholly immersed in the fluid. All that he is enabled to do, is to apply it (by some modifications and substitutions which take it out of its state of extreme generality) to the direct impulse of a vein of fluid on a plane which deflects it wholly, and thus to shew its conformity to the solution given by Daniel Bernoulli, and to observation and experience. He shows, that this impulse (independent of the deficiency arising from the plane's not being of infinite extent) is somewhat less than the weight of a column whose base is the section of the vein, and whose height is twice the fall necessary for communicating the velocity. This great philosopher and geometer concludes by saying, that he does not believe that any method can be found for solving this problem that is more direct and simple; and imagines, that if the deductions from it shall be found not to agree with experiment, we must give up all hopes of determining the resistance of fluids by theory and analytical calculus. He says *analytical calculus*; for all the physical principles on which the calculus proceeds are rigorously demonstrated, and will not admit of a doubt. There is only one hypothesis introduced in his investigation, and this is not a physical hypothesis, but a hypothesis of calculation. It is, that the quantities which determine the ratios of the second fluxions of the velocities, estimated in the directions parallel and perpendicular to the axis AC (Fig. 16.) are functions of the abscissa AP, and ordinate PM of the curve. Any person, in the least acquainted with mathematical analysis, will see, that without this supposition no analysis or calculus whatever can be instituted. But let us see what is the *physical* meaning of this hypothesis. It is simply this, that the motion of the particle M depends on its situation only. It appears impossible to form any other opinion; and if we could form such an opinion, it is as clear as day-light that the case is desperate, and that we must renounce all hopes.

We are sorry to bring our labours to this conclusion; but we are of opinion, that the only thing that remains is, for mathematicians to attach themselves with firmness and vigour to some simple cases; and, without aiming at generality, to apply M. d'Alembert's or Bernoulli's mode of procedure to the particular circumstances of the case. It is not improbable but that, in the solutions which may be obtained of these particular cases, circumstances may occur which are of a more general nature. These will be so many laws of hydraulics to be added to our present very scanty stock; and these may have points of resemblance, which will give birth to laws of still greater generality. And we repeat our expression of hopes of some success, by endeavouring to determine, in some simple cases, the *minimum possibile* of motion. The attempts of the Jesuit commentators on the *Principia* to ascertain this on the Newtonian hypothesis do them honour, and have really given us great assistance in the particular case which came through their hands.

And we should multiply experiments on the resistance of bodies. Those of the French academy are undoubtedly of inestimable value, and will always be appealed to. But there are circumstances in those experiments which render them more complicated than is proper for a general theory, and which therefore limit the conclusions which we wish to draw from them. The bodies were floating on the surface. This greatly modifies the deflections of the filaments of water, causing some to deflect laterally, which would otherwise have remained in one vertical plane; and this circumstance also necessarily produced what the academicians called the *remou*, or accumulation on the anterior part of the body, and depression behind it. This produced an additional resistance, which was measured with great difficulty and uncertainty. The effect of adhesion must also have been very considerable, and very different in the different cases; and it is of difficult calculation. It cannot perhaps be totally removed in any experiment, and it is necessary to consider it as making part of the resistance in the most important practical cases, viz. the motion of ships. Here we see that its effect is very great. Every seaman knows that the speed, even of a copper-sheathed ship, is *greatly* increased by greasing her bottom. The difference is too remarkable to admit of a doubt: nor should we be surprised at this, when we attend to the diminution of the motion of water in long pipes. A smooth pipe four and a half inches diameter, and 500 yards long, yields but one-fifth of the quantity which it ought to do independent of friction. But adhesion does a great deal which cannot be compared with friction. We see that water flowing through a hole in a thin plate will be increased in quantity fully one-third, by adding a little tube whose length is about twice the diameter of the hole. The adhesion therefore will greatly modify the action of the filaments both on the solid body and on each other, and will change both the forms of the curves and the velocities in different points; and this is a sort of objection to the only hypothesis introduced by d'Alembert. Yet it is only a sort of objection; for the effect of this adhesion, too, must undoubtedly depend on the situation of the particle.

The form of these experiments of the academy is ill-suited to the examination of the resistance of bodies wholly immersed in the fluid. The form of experiment adopted by Robins for the resistance of air, and afterwards by the Chevalier Borda for water, is free from these inconveniences, and is susceptible of equal accuracy. The great advantage of both is the exact knowledge which they give us of the velocity of the motion; a circumstance essentially necessary, and but imperfectly known in the experiments of Mariotte and others, who examined quiescent bodies exposed to the action of a stream. It is extremely difficult to measure the velocity of a stream. It is very different in its different parts. It is swiftest of all in the middle superficial filament, and diminishes as we recede from this towards the sides or bottom, and the rate of diminution is not precisely known. Could this be ascertained with the necessary precision, we should recommend the following form of experiment as the most simple, easy, economical, and accurate:

Let *a*, *b*, *c*, *d*, (Fig. 19.) be four hooks placed in a horizontal plane at the corners of a rectangular parallelogram, the sides *a b*, *c d* being parallel to the direction of the stream ABCD, and the sides *a b*, *c d* being perpendicular to it. Let the body G be fastened to an axis *e f* of stiff-tempered steel-wire, so that the surface on which the fluid is to act may be inclined to the stream in the precise angle we desire. Let this axis have hooks at its extremities, which are hitched into the loops of four equal threads, suspended from the hook *a*, *b*, *c*, *d*; and let H *e* be a fifth thread, suspended from the middle of the line joining the points of suspension *a*, *b*. Let HIK be a graduated arch, whose centre is H, and whose plane is in the direction of the stream. It is evident that the impulse on the body G will be measured (by a process well known to every mathematician) by the deviation of the thread H *e* from the vertical line H I; and this will be done without any intricacy of calculation, or any attention to the centres of gravity, of oscillation, or of percussion. These must be accurately ascertained with respect to that form in which the pendulum has always been employed for measuring the impulse or velocity of a stream. These advantages arise from the circumstance, that the axis *e f* remains always parallel to the horizon. We may be allowed to observe, by the bye, that this would have been a great improvement of the beautiful experiments of Mr Robins and Dr Hutton on the velocities of cannon-shot, and would have saved much intricate calculation, and been attended with many important advantages.

The great difficulty is, as we have observed, to measure the velocity of the stream. Even this may be done in this way with some precision. Let two floating bodies be dragged along the surface, as in the experiments of the academy, at some distance from each other laterally, so that the water between them may not be sensibly disturbed. Let a horizontal bar be attached to them, transverse to the direction of their motion, at a proper height above the surface, and let a spherical pendulum be suspended from this, or let it be suspended from four points, as here described. Now let the deviation of this pendulum be noted in a variety of velocities. This will give us the law of relation between the velocity and the deviation of the pendulum. Now, in making experiments on the resistance of bodies, let the velocity of the stream, in the very filament in which the resistance is measured, be determined by the deviation of this pendulum.

It were greatly to be wished that some more palpable argument could be found for the existence of a quantity of stagnant fluid at the anterior and posterior parts of the body. The one already given, derived from the consideration that no motion changes either its velocity or direction by finite quantities in an instant, is unexceptionable. But it gives us little information. The smallest conceivable extent of the curve FM in Fig. 18, will answer this condition, provided only that it touches the axis in some point F, and the body in some point M, so as not to make a finite angle with either. But surely there are circumstances which rigorously determine the extent of this stagnant fluid. And it appears, without doubt, that if there were no cohesion or friction, this space will have a determined ratio to the size of the body (the figures of the bodies being supposed similar). Suppose a plane surface AB, as in Fig. 11, there can be no doubt but that the figure A*a*D*b*B will in every case be similar. But if we suppose an adhesion or tenacity which is constant, this may make a change both in its extent and its form: for its constancy of form depends on the disturbing forces being always as the squares of the velocity; and this ratio of the disturbing forces is preserved, while the inertia of the fluid is the only agent and patient in the process. But when we add to this the constant (that is, invariable) disturbing force of tenacity, a change of form and dimensions must happen. In like manner, the friction, or something analogous to friction, which produces an effect proportional to the velocity, must alter this necessary ratio of the whole disturbing forces. We may conclude, that the effect of both these circumstances will be to diminish the quantity of this stagnant fluid, by licking it away externally; and to this we must ascribe the fact, that the part FAM is never perfectly stagnant, but is generally disturbed by a whirling motion. We may also conclude, that this stagnant fluid will be more incurvated between F and M than it would have been, independent of tenacity and friction; and that the arch LR will, on the contrary, be less incurvated.—And, lastly, we may conclude, that there will be something opposite to pressure, or something which we may call *abstraction*, exerted on the posterior part of the body which moves in a tenacious fluid, or is exposed to the stream of such a fluid; for the stagnant fluid LCR adheres to the surface LC; and the passing fluid tends to draw it away both by its tenacity and by its friction. This must augment the apparent impulse of the stream on such a body; and it must greatly augment the resistance, that is, the motion lost by this body in its progress through the tenacious fluid:

for the body must drag along with it this stagnant fluid, and drag it in opposition to the tenacity and friction of the surrounding fluid. The effect of this is most remarkably seen in the resistances to the motion of pendulums; and the Chevalier Buat, in his examination of Newton's experiments, clearly shews that this constitutes the greatest part of the resistance.

This most ingenious writer has paid great attention to this part of the process of nature, and has laid the foundation of a theory of resistance entirely different from all the preceding. We cannot abridge it; and it is too imperfect in its present condition to be offered as a body of doctrine: but we hope that the ingenious author will prosecute the subject.

WE cannot conclude this dissertation (which we acknowledge to be very unsatisfactory and imperfect) better, than by giving an account of some experiments of the Chevalier Buat, which seem of immense consequence, and tend to give us very new views of the subject. Mr Buat observed the motion of water issuing from a glass cylinder through a narrow ring formed by a bottom of smaller diameter; that is, the cylinder was open at both ends, and there was placed at its lower end a circle of smaller diameter, by way of bottom, which left a ring all around. He threw some powdered sealing-wax into the water, and observed with great attention the motion of its small particles. He saw those which happened to be in the very axis of the cylinder descend along the axis with a motion pretty uniform, till they came very near the bottom; from this they continued to descend very slowly, till they were almost in contact with the bottom; they then deviated from the centre, and approached the orifice in straight lines and with an accelerated motion, and at last darted into the orifice with great rapidity. He had observed a thing similar to this in a horizontal canal, in which he had set up a small board like a dam or bar, over which the water flowed. He had thrown a gooseberry into the water, in order to measure the velocity at the bottom, the gooseberry being a small matter heavier than water. It approached the dam uniformly till about three inches from it. Here it almost stood still, but it continued to advance till almost in contact. It then rose from the bottom along the inside of the dam with an accelerated motion, and quickly escaped over the top.

Hence he concluded, that the water which covers the anterior part of the body exposed to the stream is not perfectly stagnant, and that the filaments recede from the axis in curves, which converge to the surface of the body as different hyperbolas converge to the same assymptote, and that they move with a velocity continually increasing, till they escape round the sides of the body.

He had established (by a pretty reasonable theory, confirmed by experiment) a proposition concerning the pressure which water in motion exerts on the surface along which it glides, viz. *that the pressure is equal to that which it would exert if at rest* minus *the weight of the column whose height would produce the velocity of the passing stream.* Consequently the pressure which the stream exerts on the surface perpendicularly exposed to it will depend on the velocity with which it glides along it, and will diminish from the centre to the circumference. This, says he, may be the reason why the impulse on a plane wholly immersed is but one half of that on a plane which deflects the whole stream.

He contrived a very ingenious instrument for examining this theory. A square brass plate ABGF (Fig. 20.) was pierced with a great number of holes, and fixed in the front of a shallow box represented edgewise in Fig. 21. The back of this box was pierced with a hole C, in which was inserted the tube of glass CDE, bent square at D. This instrument was exposed to a stream of water, which beat on the brass plate. The water having filled the box through the holes, stood at an equal height in the glass tube when the surrounding water was stagnant; but when it was in motion, it always stood in the tube above the level of the smooth water without, and thus indicated the pressure occasioned by the action of the stream.

When the instrument was not wholly immersed, there was always a considerable accumulation against the front of the box, and a depression behind it. The water before it was by no means stagnant: indeed it should not be, as Mr Buat observes; for it consists of the water which was escaping on all sides, and therefore upwards from the axis of the stream, which meets the plate perpendicularly in C considerably under the surface. It escapes upwards; and if the body were sufficiently immersed, it would escape in this direction almost as easily as laterally. But, in the present circumstances, it heaps up, till the elevation occasions it to fall off sidewise as fast as it is renewed. When the instrument was immersed more than its semidiameter under the surface, the water still rose above the level, and there was a great depression immediately behind this elevation. In consequence of this difficulty of escaping upwards, the water flows off laterally; and if the horizontal dimensions of the surface is great, this lateral efflux becomes more difficult, and requires a greater accumulation. From this it happens, that the resistance of broad surfaces equally immersed is greater than in the proportion of the breadth. A plane of two feet wide and one foot deep, when it is not completely immersed, will be more resisted than a plane two feet deep and one foot wide; for there will be an accumulation against both; and even if these were equal in height, the additional surface will be greatest in the widest body; and the elevation will be greater, because the lateral escape is more difficult.

The circumstances chiefly to be attended to are these:

The pressure on the centre was much greater than towards the border, and, in general, the height of the water in the tube DE was more than $\frac{4}{3}$ of the height necessary for producing the velocity when only the central hole was open. When various holes were opened at different distances from the centre, the height of the water in DH (Fig. 23.) continually diminished as the hole was nearer the border. At a certain distance from the border the water at E was level with the surrounding water, so that no pressure was exerted on that hole. But the most unexpected and remarkable circumstance was, that in great velocities, the holes at the very border, and even to a small distance from it, not only sustained no pressure, but even gave out water; for the water in the tube was lower than the surrounding water. Mr Buat calls this a *non-pression*. In a case in which the velocity of the stream was three feet, and the pressure on the central hole caused the water in the vertical tube to stand 33 lines or $\frac{33}{12}$ of an inch above the level of the surrounding smooth water, the action on a hole at the lower corner of the square caused it to stand 12 lines lower than the surrounding water. Now the velocity of the stream in this experiment was 36 inches per second. This requires 21½ lines for its productive fall; whereas the pressure on the central hole was 33. This approaches to the pressure on a surface which deflects it wholly. The intermediate holes gave every variation of pressure, and the diminution was more rapid as the holes were nearer the edge; but the law of diminution could not be observed.

This is quite a new and most unexpected circumstance in the action of fluids on solid bodies, and renders the subject more intricate than ever; yet it is by no means inconsistent with the genuine principles of hydrostatics or hydraulics. In as far as M. Buat's proposition concerning the pressure of moving fluids is true, it is very reasonable to say, that when the lateral velocity with which the fluid tends to escape exceeds the velocity of percussion, the height necessary for producing this velocity must exceed that which would produce the other, and a non-pression must be observed. And if we consider the forms of the lateral filaments near the edge of the body, we see that the concavity of the curve is turned towards the body, and that the centrifugal forces tend to diminish their pressure on the body. If the middle alone were struck with a considerable velocity, the water might even rebound, as is frequently observed. This *actual* rebounding is here prevented by the surrounding water, which is moving with the same velocity: but the pressure may be almost annihilated by the tendency to rebound of the inner filaments.

Part (and perhaps a considerable part) of this apparent non-pression is undoubtedly produced by the tenacity of the water, which licks off with it the water lying in the hole. But, at any rate, this is an important fact, and gives great value to these experiments. It gives a key to many curious phenomena in the resistance of fluids; and the theory of Mr Buat deserves a very serious consideration. It is all contained in the two following propositions:

1. "*If by any cause whatever, a column of fluid, whether making part of an indefinite fluid, or contained in solid canals, come to move with a given velocity, the pressure which it exerted laterally before its motion, either on the adjoining fluid or on the sides of the canal, is diminished by the weight of a column having the height necessary for communicating the velocity of the motion.*

2. "*The pressure on the centre of a plane surface, perpendicular to the stream, and wholly immersed in it, is $\frac{3}{2}$ of the weight of a column having the height necessary for communicatiug the velocity. For 33 is $\frac{3}{2}$ of $21\frac{1}{2}$.*"

He attempted to ascertain the medium pressure on the whole surface, by opening 625 holes dispersed all over it. With the same velocity of current, he found the height in the tube to be 29 lines, or 7½ more than the height necessary for producing the velocity. But he justly concluded this to be too great a measure, because the holes were ¼ of an inch from the edge: had there been holes at the very edge, they would have sustained a non-pression, which would have diminished the height in the tube very considerably. He exposed to the same stream a conical funnel, which raised the water to 34 lines. But this could not be considered as a measure of the pressure on a plane solid surface; for the central water was undoubtedly scooped out, as it were, and the filaments much more deflected than they would have been by a plane surface. Perhaps something of this happened even in every small hole in the former experiments. And this suggests some doubt as to the accuracy of the measurement of the pressure, and of the velocity of a current by Mr Pitot's tube. It surely renders some corrections absolutely necessary. It is a fact, that when exposed to a vein of fluid coming through a short passage, the water in the tube stands on a level with that in the reservoir. Now we know that the velocity of this stream *does not* exceed what would be produced by a fall equal to $\frac{98}{100}$ of the head of water in the reservoir. Mr Buat made many valuable observations and improvements on this most useful instrument, which will be taken notice of in the articles RIVERS and WATER-WORKS.

Mr Buat, by a scrupulous attention to all the circumstances, concludes, that the medium of pressure on the whole surface is equal to $\frac{25.5}{21.5}$ of the weight of a column, having the surface for its base, and the productive fall for its height. But we think that there is an uncertainty in this conclusion; because the height of the water in the vertical tube was undoubtedly augmented by an hydrostatical pressure arising from the accumulation of water above the body, which was exposed to the stream.

Since the pressures are as the squares of the velocities, or as the heights h which produce the velocities, we may express this pressure by the symbol $\frac{25.5}{21.5}h$, or 1.186 h, or $m\,h$, the value of m being 1.186. This exceeds considerably the results of the experiments of the French academy. In these it does not appear that m sensibly exceeds unity. Note, that in these experiments the body was moved through still water; here it is exposed to a stream. These are generally supposed to be equivalent, on the authority of the third law of motions. We shall by and by see some causes of difference.

The writers on this subject seem to think their task completed when they have considered the action of the fluid on the anterior part of the body, or that part of it which is before the broadest section, and have paid little or no attention to the hinder part. Yet those who are most interested in the subject, the naval architects, seem convinced that it is of no less importance to attend to the form of the hinder part of a ship. And the universal practice of all nations has been to make the hinder part more acute than the fore-part. This has undoubtedly been deduced from experience; for it is in direct opposition to any notions which a person would naturally form on this subject. Mr Buat therefore thought it very necessary to examine the action of the water on the hinder part of a body by the same method. And previous to this examination, in order to acquire some scientific notions of the subject, he made the following very curious and instructive experiment:

Two little conical pipes AB (Fig. 22.) were inserted into the upright side of a prismatic vessel. They were an inch long, and their diameters at the inner and outer ends were five and four lines. A was 57 lines under the surface, and B was 73. A glass syphon was made of the shape represented in the figure, and its internal diameter was 1¼ lines. It was placed with its mouth in the axis, and even with the base of the conical pipe. The pipes being shut, the vessel was filled with water, and it was made to stand on a level in the two legs of the syphon, the upper part being full of air. When this syphon was applied to the pipe A, and the water running freely, it rose 32 lines in the short leg, and sunk as much in the other. When it was applied to the pipe B, the water rose 41 lines in the one leg of the syphon, and sunk as much in the other.

He reasons in this manner from the experiment. The ring comprehended between the end of the syphon and the sides of the conical tube being the narrowest part of the orifice, the water issued with the velocity corresponding to the height of the water in the vessel above the orifice, diminished for the contraction. If therefore the cylinder of water immediately before the mouth of the syphon issued with the same velocity, the tube would be emptied through a height equal to this HEAD OF WATER (*charge.*) If, on the contrary, this cylinder of water, immediately before the mouth of the syphon, were stagnant, the water in it would exert its full pressure on the mouth of the syphon, and the water in the syphon would be level with the water in the vessel. Between these extremes we must find the real state of the case, and we must measure the force of non-pressure by the rise of the water in the syphon.

We see that in both experiments it bears an accurate proportion to the depth under the surface. For $57 : 73 = 32 : 41$ very nearly. He therefore estimates the non-pressure to be $\frac{56}{100}$ of the height of the water above the orifice.

We are disposed to think that the ingenious author has not reasoned accurately from the experiment. In the first place, the force indicated by the experiment, whatever be its origin, is certainly double of what he supposes; for it must be measured by the sum of the rise of the water in one leg, and its depression in the other, the weight of the air in the bend of the syphon being neglected. It is precisely analogous to the force acting on the water oscillating in a syphon, which is acknowledged to be the sum of the elevation and depression. The force indicated by the experiment therefore is [illegible] of the height of the water above the orifice. The force exhibited in this experiment bears a still greater proportion to the productive height; for it is certain that the water *did not* issue with the velocity acquired by the fall from the surface, and probably did not exceed [illegible] of it. The effect of contraction must have been considerable and uncertain. The velocity should have been measured both by the amplitude of the jet and by the quantity of water discharged. In the next place, we apprehend that much of the effect is produced by the tenacity of the water, which drags along with it the water which would have slowly issued from the syphon, had the other end not dipped into the water of the vessel. We know, that if the horizontal part of the syphon had been continued far enough, and if no retardation were occasioned by friction, the column of water in the upright leg would have accelerated like any heavy body; and when the last of it had arrived at the bottom of that leg, the whole in the horizontal part would be moving with the velocity acquired by falling from the surface. The water of the vessel which issues through the surrounding ring very quickly, acquires a much greater velocity than what the water descending in the syphon would acquire in the same time, and it drags this last water along with it both by tenacity and friction, and it drags it out till its action is opposed by the want of equilibrium produced in the syphon, by the elevation in the one leg and the depression in the other. We imagine that little can be concluded from the experiment with respect to the real non-pressure. Nay, if the sides of the syphon be supposed infinitely thin, so that there would be no curvature of the filaments of the surrounding water at the mouth of the syphon, we do not very distinctly see any source of non-pressure: For we are not altogether satisfied with the proof which Mr Buat offers for this measure of the pressure of a stream of fluid gliding along a surface, *and obstructed by friction or any other cause.* We imagine that passing water in the present experiment would be a little retarded by accelerating continually the water descending in the syphon, and renewed a-top, supposing the upper end open; because this water would not of itself acquire more than half this velocity. It however drags it out, till it not only resists with a force equal to the weight of the whole vertical column, but even exceeds it by $\frac{12}{100}$. This it is able to do, because the whole pressure by which the water issues from an orifice has been shown (by Daniel Bernoulli) to be equal to twice this weight. We therefore consider this beautiful experiment as chiefly valuable, by giving us a measure of the tenacity of the water; and we wish that it were repeated in a variety of depths, in order to discover what relation the force exerted bears to the depth. It would seem that the tenacity, being a certain determinate thing, the proportion of 100 to 112 would not be constant; and that the observed ratio would be made up of two parts, one of them constant, and the other proportional to the depth under the surface.

But still this experiment is intimately connected with the matter in hand; and this apparent non-pressure on the hinder part of a body exposed to a stream, from whatever causes it proceeds, does operate in the action of water on this hinder part, and must be taken into the account.

We must therefore follow the Chevalier de Buat in his discussions on this subject. A prismatic body, having its prow and poop equal and parallel surfaces, and plunged horizontally into a fluid, will require a force to keep it firm in the direction of its axis precisely equal to the difference between the real pressures exerted on its prow and poop. If the fluid is at rest, this difference will be nothing, because the opposite dead pressures of the fluid will be equal: but in a stream, there is superadded to the dead pressure on the prow the active pressure arising from the deflection of the filaments of this fluid.

If the dead pressure on the poop remained in its full intensity by the perfect stagnation of the water behind it, the whole sensible pressure on the body would be the active pressure only on the prow, represented by $m\,h$. If, on the other hand, we could suppose that the water behind the body moved continually away from it (being renewed laterally) with the velocity of the stream, the dead pressure would be entirely removed from its poop, and the whole sensible pressure, or what must be opposed by some external force, would be $m\,h+h$. Neither of these can happen; and the real state of the case must be between these extremes.

The following experiments were tried: The perforated box with its vertical tube was exposed to the stream, the brass plate being turned down the stream. The velocity was again 36 inches per second.

The central hole A alone being opened, gave a non-pressure of - -	13 lines.
A hole B, ⅝ of an inch from the edge, gave	15
A hole C near the surface -	15.7
A hole D, at the lower angle -	15.3

Here it appears that there is a very considerable non-pressure, increasing from the centre to the border. This increase undoubtedly proceeds from the greater lateral velocity with which the water is gliding in from the sides. The water behind was by no means stagnant, although moving off with a much smaller velocity than that of the passing stream, and it was visibly removed from the sides, and gradually licked away at its further extremity.

Another box, having a greater number of holes, all open, indicated a medium of non-pressure equal to 13⅓ lines.

Another of larger dimensions, but having fewer holes, indicated a non-pressure of 12⅛.

But the most remarkable, and the most important phenomena, were the following:

The first box was fixed to the side of another box, so that, when all was made smooth, it made a perfect cube, of which the perforated brass plate made the poop.

The apparatus being now exposed on the stream, with the perforated plate looking down the stream,

The hole A indicated a non-impression	=7.2
B	8
C	6

Here was a great diminution of the non-pressions produced by the distance between the prow and the poop.

This box was then fitted in the same manner, so as to make the poop of a box three feet long. In this situation the non-pressures were as follow:

Hole A	1.5
B	3.2

The non-pressions were still farther diminished by this increase of length.

The box was then exposed with all the holes open, in three different situations:

1st, Single, giving a non-pressure	13.1
2d, Making the poop of a cube	5.3
3d, Making the poop of a box three feet long	3.0

Another larger box:

1st, Single	12.2
2d, Poop of a cube	5.
3d, Poop of the long box	3.2

These are most valuable experiments. They plainly show how important it is to consider the action on the hinder part of the body. For the whole impulse or resistance, which must be withstood or overcome by the external force, is the sum of the active pressure on the fore-part, and of the non-pressure on the hinder-part; and they show that this does not depend solely on the form of the prow and poop, but also, and perhaps chiefly, on the length of the body. We see that the non-pressure on the hinder-part was prodigiously diminished (reduced to one-fourth) by making the length of the body triple of the breadth. And hence it appears, that merely lengthening a ship, without making any change in the form either of her prow or her poop, will greatly diminish the resistance to her motion through the water; and this increase of length may be made by continuing the form of the midship frame in several timbers along the keel, by which the capacity of the ship, and her power of carrying sail, will be greatly increased, and her other qualities improved, while her speed is augmented.

It is surely of importance to consider a little the physical cause of this change. The motions are extremely complicated, and we must be contented if we can but perceive a few leading circumstances.

The water is turned aside by the anterior part of the body, and the velocity of the filaments is increased, and they acquire a divergent motion, by which they also push aside the surrounding water. On each side of the body, therefore, they are moving in a divergent direction, and with an increased velocity. But as they are on all sides pressed by the fluid without them, their motions gradually approach parallelism, and their velocities to an equality with the stream. The progressive velocity, or that in the direction of the stream, is checked, at least at first. But since we observe the filaments constipated round the body, and that they are not deflected at right angles to their former direction, it is plain that the real velocity of a filament in its oblique path is augmented. We always observe, that a stone lying in the sand, and exposed to the wash of the sea, is laid bare at the bottom, and the sand is generally washed away to some distance all round. This is owing to the increased velocity of the water which comes into contact with the stone. It takes up more sand than it can keep floating, and it deposites it at a little distance all around, forming a little bank, which surrounds the stone at a small distance. When the filaments of water have passed the body, they are pressed by the ambient fluid into the place which it has quitted, and they glide round its stern, and fill up the space behind. The more divergent and the more rapid they are, when about to fall in behind, the more of the circumambient pressure must be employed to turn them into the trough behind the body, and less of it will remain to press them to the body itself. The extreme of this must obtain when the stream is obstructed by a thin plane only. But when there is some distance between the prow and the poop, the divergency of the filaments which had been turned aside by the prow is diminished by the time that they have come abreast of the stern, and should turn in behind it. They are therefore more readily made to converge behind the body, and a more considerable part of the surrounding pressure remains unexpended, and therefore presses the water against the stern; and it is evident that this advantage must be so much the greater as the body is longer. But the advantage will soon be susceptible of no very considerable increase: for the lateral, and divergent, and accelerated filaments, will soon become so nearly parallel and equally rapid with the rest of the stream, that a great increase of length will not make any considerable change in these particulars; and it must be accompanied with an increase of friction.

These are very obvious reflections. And if we attend minutely to the way in which the almost stagnant fluid behind the body is expended and renewed, we shall see all these effects confirmed and augmented. But as we cannot say any thing on this subject that is precise, or that can be made the subject of computation, it is needless to enter into a more minute discussion. The diminution of the non-pressure towards the centre most probably arises from the smaller force which is necessary to be expended in the inflection of the lateral filaments, already inflected in some degree, and having their velocity diminished. But it is a subject highly deserving the attention of the mathematicians; and we presume to invite them to the study of the motions of these lateral filaments, passing the body, and pressed into its wake by forces which are susceptible of no difficult investigation. It seems highly probable, that if a prismatic box, with a square stern, were fitted with an addition precisely shaped like the water which would (abstracting tenacity and friction) have been stagnant behind it, the quantity of non-pression would be the smallest possible. The mathematician would surely discover circumstances which would furnish some maxims of construction for the hinder part as well as for the prow. And as his speculations on this last have not been wholly fruitless, we may expect advantages from his attention to this part, so much neglected.

In the meantime, let us attend to the deductions which Mr de Buat has made from his few experiments.

When the velocity is three feet per second, requiring the productive height 21.5 lines, the heights corresponding to the non-pressure on the poop of a thin plane is 14.41 lines (taking in several circumstances of judicious correction, which we have not mentioned), that of a foot cube is 5.83, and that of a box of triple length is 3.31.

Let q express the variable ratio of these to the height producing the velocity, so that $q\ h$ may express the non-pressure in every case; we have,

For a thin plane - - - -	$q=0.67$
a cube - - - - -	0.271
a box = 3 cubes - - - -	0.153

It is evident that the value of q has a dependence on the proportion of the length, and the transverse section of the body. A series of experiments on prismatic bodies showed Mr de Buat that the deviation of the filaments was similar in similar bodies, and that this obtained even in dissimilar prisms, when the lengths were as the square-roots of the tranverse sections. Although therefore the experiments were not sufficiently numerous for deducing the precise law, it seemed not impossible to derive from them a very useful approximation. By a dexterous comparison he found that if l expresses the length of the prism, and s the area of the transverse section, and L expresses the common logarithm of the quantity to which it is prefixed, we shall express the non-pressure pretty accurately by the formula $\frac{1}{q} = \mathrm{L}\left(1.42 \frac{l}{\sqrt{s}}\right)$.

Hence arises an important remark, that when the height corresponding to the non-pression is greater than $\sqrt{s}$, and the body is little immersed in the fluid, there will be a void behind it. Thus a surface of a square inch, just immersed in a currrent of three feet per second, will have a void behind it. A foot square will be in a similar condition when the velocity is 12 feet.

We must be careful to distinguish this non-pressure from the other causes of resistance, which are always necessarily combined with it. It is superadditive to the active impression on the prow, to the statical pressure of the accumulation a-head of the body, the statical pressure arising from the depression behind it, the effects of friction, and the effects of tenacity. It is indeed next to impossible to estimate them separately, and many of them are actually combined in the measures now given. Nothing can determine the pure non-pressures till we can ascertain the motions of the filaments.

Mr de Buat here takes occasion to controvert the universally adopted maxim, that the pressure occasioned by a stream of fluid on a fixed body is the same with that on a body moving with equal velocity in a quiescent fluid. He repeated all these experiments with the perforated box in still water. The general distinction was, that both the pressures and the non-pressure in this case were less, and that the odds were chiefly to be observed near the edges of the surface. The general factor of the pressure of a stream on the anterior surface was $m = 1.186$; but that on a moving body through a still fluid is only $m = 1$ He observed no non-pressure even at the very edge of the prow, but even a sensible pressure. The pressure, therefore, or resistance, is more equally diffused over the surface of the prow than the impulse is.—He also found that the resistances diminished in a less ratio than the squares of the velocities, especially in small velocities.

The non-pressures increased in a greater ratio than the squares of the velocities. The ratio of the velocities to a small velocity of $2\frac{1}{5}$ inches per second increased geometrically, the value of q increased arithmetically; and we may determine q for any velocity V by this proportion

$L\,\frac{55}{2.2} : L\,\frac{V}{22} = 0.5 : q$, and $q = \frac{L\frac{V}{2.2}}{2.8}$. That is, let the common logarithm of the velocity, divided by $2\frac{1}{5}$, be considered as a common number; divide this common number by $2\frac{8}{10}$, the quotient is q, which must be multiplied by the productive height. The product is the pressure.

When Pitot's tube was exposed to the stream, we had $m = 1$; but when it is carried through still water, m is $= 1.22$. When it was turned from the stream, we had $q = 0.157$; but when carried through still water, q is $= 0.138$. A remarkable experiment.

When the tube was moved laterally through the water, so that the motion was in the direction of the plane of its mouth, the non-pressure was $= 1$. This is one of his chief arguments for his theory of non-pression. He does not give the detail of the experiment, and only inserts the result in his table.

As a body exposed to a stream deflects the fluid, heaps it up, and increases its velocity; so a body moved through a still fluid turns it aside, causes it to swell up before it, and gives it a real motion alongside of it in the opposite direction. And as the body exposed to a stream has a quantity of fluid almost stagnant both before and behind, so a body moved through a still fluid carries before it and drags after it a quanity of fluid, which accompanies it with nearly an equal velocity. This addition to the quantity of matter in motion must make a diminution of its velocity; and this forms a very considerable part of the observed resistance.

We cannot, however, help remarking, that it would require very distinct and strong proof indeed to overturn the common opinion, which is founded on our most certain and simple conceptions of motion, and on a law of nature to which we have never observed an exception. M. de Buat's experiments, though most judiciously contrived, and executed with scrupulous care, are by no means of this kind. They were, of absolute necessity, very complicated; and many circumstances, impossible to avoid or to appreciate, rendered the observation, or at least the comparison, of the velocities, very uncertain.

We can see but two circumstances which do not admit of an easy or immediate comparison in the two states of the problem. When a body is exposed to a stream, *in our experiments*, in order to have an impulse made on it, there is a force tending to move the body backwards, independent of the real impulse or pressure occasioned by the deflection of the stream. We cannot have a stream except in consequence of a sloping surface. Suppose a body floating on this stream. It will not only sail down *along with the stream*, but it will sail *down the stream*, and will therefore go faster along the canal than the stream does; for it is floating on an inclined plane; and if we examine it by the laws of hydrostatics, we shall find, that, besides its own tendency to *slide* down this inclined plane, there is an odds of hydrostatical pressure, which *pushes* it down this plane.

It will therefore go along the canal *faster* than the stream. For this acceleration depends on the difference of pressure at the two ends, and will be more remarkable as the body is larger, and especially as it is longer. This may be distinctly observed. All floating bodies go into the stream of the river, because there they find the smallest obstruction to the acquisition of this motion along the inclined plane; and when a number of bodies are thus floating down the stream, the largest and longest outstrip the rest. A log of wood floating down in this manner may be observed to make its way very fast among the chips and saw-dust which float alongside of it.

Now when, in the course of our experiments, a body is supported against the action of the stream, and the impulse is measured by the force employed to support it, it is plain that part of this force is employed to act against that tendency which the body has to outstrip the stream. This does not appear in our experiment, when we move a body with the velocity of this stream through still water having a horizontal surface.

The other distinguishing circumstance is, that the retardations of a stream arising from friction are found to be nearly as the velocities. When, therefore, a stream moving in a limited canal is checked by a body put in its way, the diminution of velocity occasioned by the friction of the stream having already produced its effect, the impulse is not affected by it; but when the body puts the still water in motion, the friction of the bottom produces some effect, by retarding the recess of the water. This, however, must be next to nothing.

The chief difference will arise from its being almost impossible to make an exact comparison of the velocities: for when a body is moved against the stream, the relative velocity is the same in all the filaments. But when we expose a body to a stream, the velocity of the different filaments is not the same; because it decreases from the middle of the stream to the sides.

M. Buat found the total sensible resistance of a plate 12 inches square, and measured, not by the height of water in the tube of the perforated box, but by weights acting on the arm of a balance, having its centre 15 inches under the surface of a stream moving three feet per second, to be 19.46 pounds; that of a cube of the same dimensions was 15.22; and that of a prism three feet long was 13.87; and that of a prism six feet long was 14.27. The three first agree extremely well with the determination of m and q, by the experiments with the perforated box. The total resistance of the last was undoubtedly much increased by friction, and by the retrograde force of so long a prism floating on an inclined stream. This last by computation is 0.223 pounds; this added to $h\,(m + q)$, which is 13.59, gives 13.81, leaving 0.46 for the effect of friction.

If the same resistances be computed on the supposition that the body moves in still water, in which case we have $m = 1$, and q for a thin plate $= 0.433$; and if q be computed for the lengths of the other two bodies by the formula $\frac{1}{q} = \text{L}\,1.42 + \frac{l}{\sqrt{s}}$, we shall get for the resistances 14.94; 12.22; and 11.49.

Hence M. Buat concludes, that the resistances in these two states are nearly in the ratio of 13 to 10. This, he thinks, will account for the difference observed in the experiments of different authors.

M. Buat next endeavours to ascertain the quantity of water which is made to adhere in some degree to a body which is carried along through still water, or which remains nearly stagnant in the midst of a stream. He takes the sum of the motions in the direction of the stream, viz. the sum of the actual motions of all those particles which have lost part of their motion, and he divides this sum by the general velocity of the stream. The quotient is equivalent to a certain quantity of water perfectly stagnant round the body. Without being able to determine this with precision, he observes, that it augments as the resistance diminishes; for in the case of a longer body, the filaments are observed to converge to a greater distance behind the body. The stagnant mass a-head of the body is more constant; for the deflection and resistance at the prow are observed not to be affected at the length of the body. M. Buat, by a very nice analysis of many circumstances, comes to this conclusion—that the whole quantity of fluid, which in this manner accompanies the solid body, remains the same whatever is the velocity. He might have deduced it at once, from the consideration that the curves described by the filaments are the same in all velocities.

He then relates a number of experiments made to ascertain the absolute quantity thus made to accompany the body. These were made by causing pendulums to oscillate in fluids. Newton had determined the resistances to such oscillation by the diminution of the arches of vibration. M. Buat determines the quantity of dragged fluid by the increase of their duration; for this stagnation or dragging is in fact adding a quantity of matter to be moved, without any addition to the moving force. It was ingeniously observed by Newton, that the time of oscillation was not sensibly affected by the resistance of the fluid: a compensation, almost complete, being made by the diminution of the arches of vibration; and experiment confirmed this. If, therefore, a great augmentation of the time of vibration be observed, it must be ascribed to the additional quantity of matter which is thus dragged into motion, and it may be employed for its measurement. Thus, let a be the length of a pendulum swinging seconds in vacuo, and l the length of a second's pendulum swinging in a fluid. Let p be the weight of the body in the fluid, and P the weight of the body displaced by it; $P + p$ will express its weight in vacuo, and $\frac{P+p}{p}$ will be the ratio of these weights. We shall therefore have $\frac{P+p}{p}=\frac{a}{l}$ and $l=\frac{a\,p}{P+p}$.

Let n express the sum of the fluid displaced, and the fluid dragged along, n being a greater number than unity, to be determined by experiment. The mass in motion is no longer $P+p$, but $P+n\,p$, while its weight in the fluid is still p. Therefore we must have $l=\frac{a\,p}{n\,P+p}$ $=\frac{a}{\frac{nP}{p}+1}$, and $n=\frac{p}{P}\left(\frac{a}{l}-1\right)$

A prodigious number of experiments made by M. Buat on spheres vibrating in water gives values of n, which were very constant, namely, from 1.5 to 1.7; and by considering the circumstances which accompanied the variations of n (which he found to arise chiefly from the curvature of the path described by the ball), he states the mean value of the number n at 1.583. So that a sphere in motion drags along with it about $\frac{6}{10}$ of its own bulk of fluid with a velocity equal to its own.

He made similar experiments with prisms, pyramids, and other bodies, and found a complete confirmation of his assertion, that prisms of equal lengths and sections, though dissimilar, dragged equal quantities of fluid; that similar prisms and prisms not similar, but whose lengths were as the square root of their sections, dragged quantities proportional to their bulks.

He found a general value of n for prismatic bodies, which alone may be considered as a valuable truth; namely, that $n=0.705\,\frac{\sqrt{s}}{l}+1.13$.

From all these circumstances, we see an intimate connection between the pressures, non-pressures, and the fluid dragged along with the body. Indeed this is immediately deducible from the first principles; for what Mr Buat calls the *dragged fluid* is in fact a certain portion of the whole change of motion produced in the direction of the body's motion.

It was found, that with respect to thin planes, spheres, and pyramidal bodies of equal bases, the resistances were inversely as the quantities of fluid dragged along.

The intelligent reader will readily observe, that these views of the Chevalier Buat are not so much discoveries of new principles as they are classifications of consequences, which may all be deduced from the general principles employed by D'Alembert and other mathematicians. But they greatly assist us in forming notions of different parts of the procedure of nature in the mutual action of fluids and solids on each other. This must be very acceptable in a subject which it is by no means probable that we shall be able to investigate with mathematical precision. We have given an account of these last observations, that we may omit nothing of consequence that has been written on the subject; and we take this opportunity of recommending the *Hydraulique* of Mr Buat as a most ingenious work, containing more original, ingenious, and practically useful thoughts, than all the performances we have met with. His doctrine of *the principle of uniform motion of fluids in pipes and open canals* will be of immense service to all engineers, and enable them to determine with sufficient precision the most important questions in their profession—questions which at present they are hardly able to guess at. See the articles RIVERS and WATER-WORKS in this volume.

The only circumstance which we have not noticed in detail, is the change of resistance produced by the void, or tendency to a void, which obtains behind the body; and we omitted a particular discussion, merely because we could say nothing sufficiently precise on the subject. Persons not accustomed to the discussions in the physico-mathematical sciences, are apt to entertain doubts or false notions connected with this circumstance, which we shall attempt to remove; and with this we shall conclude this dissertation.

If a fluid were perfectly incompressible, and were contained in a vessel incapable of extension, it is impossible that any void could be formed behind the body; and in this case it is not very easy to see how motion could be performed in it. A sphere moved in such a medium could not advance the smallest distance, unless *some* particles of the fluid, in filling up the space left by it, moved with a velocity next to infinite. Some degree of compressibility, however small, seems necessary. If this be insensible, it may be rigidly demonstrated, that an external force of compression will make no *sensible* change in the internal motions, or in the resistances. This indeed is not obvious, but is an immediate consequence of the *quaquaversum* pressure of fluids. As much as the pressure is augmented by the external compressions in one side of a body, so much is it augmented on the other side; and the same must be said of every particle. Nothing more is necessary for securing the same motions by the same partial and internal forces; and this is fully verified by experiment. Water remains equally fluid under any compressions. In some of Sir Isaac Newton's experiments, balls of four inches diameter were made so light as to preponderate in water only three grains. These balls descended in the same manner as they would have descended in a fluid where the resistance was equal in every part; yet, when they were near the bottom of a vessel nine feet deep, the compression round them was at least 2400 times the moving force; whereas, when near the top of the vessel, it was not above 50 or 60 times.

But in a fluid sensibly compressible, or which is not confined, a void may be left behind the body. Its motion may be so swift that the surrounding pressure may not suffice for filling up the deserted space; and, in this case, a statical pressure will be added to the resistance. This may be the case in a vessel or pond of water having an open surface exposed to the finite or limited pressure of the atmosphere. The question now is, whether the resistance will be increased by an increase of external pressure? Supposing a sphere moving near the surface of water, and another moving equally fast at four times the depth. If the motion be so swift that a void is formed in both cases, there is no doubt but that the sphere which moves at the greatest depth is most resisted by the pressure of the water. If there is no void in either case, then, because the quadruple depth would cause the water to flow in with only a double velocity, it would seem that the resistance would be greater; and indeed the water flowing in laterally with a double velocity produces a quadruple non-pressure.—But, on the other hand, the pressure at a small depth may be insufficient for preventing a void, while that below effectually prevents it; and this was observed in some experiments of Chevalier de Borda. The effect, therefore, of greater immersion, or of greater compression, in an elastic fluid, does not follow a precise ratio of the pressure, but depends partly on absolute quantities. It cannot, therefore, be stated by any very simple formula what increase or diminution of resistance will result from a greater depth; and it is chiefly on this account that experiments made with models of ships and mills are not conclusive with respect to the performance of a large machine of the same proportions, without corrections, sometimes pretty intricate. We assert, however, with great confidence, that this is of all methods the most exact, and infinitely more certain than any thing that can be deduced from the most elaborate calculation from theory. If the resistances at all depths be equal, the proportionality of the total resistance to the body is exact, and perfectly conformable to observation. It is only in great velocities where the depth has any material influence, and the influence is not near so considerable as we should, at first sight, suppose; for, in estimating the effect of immersion, which has a relation to the difference of pressure, we must always take in the pressure of the atmosphere; and thus the pressure at 33 feet deep is not 33 times the pressure at one foot deep, but only double, or twice as great. The atmospheric pressure is omitted only when the resisted plane is at the very surface. D'Ulloa, in his *Examino Maritimo*, has introduced an equation expressing this relation; but, except with very limited conditions, it will mislead us prodigiously. To give a general notion of its foundation, let AB (Fig. 23.) be the section of a plane moving through a fluid in the direction CD, with a known velocity. The fluid will be heaped up before it above its natural level CD, because the water will not be pushed before it like a solid body, but will be pushed aside. And it cannot acquire a lateral motion any other way than by an accumulation, which will diffuse itself in all directions by the law of undulatory motion. The water will also be left lower behind the plane, because time *must* elapse before the pressure of the water behind can make it fill the space. We may acquire some notion of the extent of both the accumulation and depression in this way. There is a certain depth CF (= $\frac{v^2}{2\varphi}$, where v is the velocity, and φ the accelerating power of gravity) under the surface, such that water would flow through a hole at F with the velocity of the plane's motion. Draw a horizontal line FG. The water will certainly touch the plane in G, and we may suppose that it touches it no higher up. Therefore there will be a hollow, such as CGE. The elevation HE will be regulated by considerations nearly similar. ED must be equal to the velocity of the plane, and HE must be its productive height. Thus, if the velocity of the plane be one foot per second, HE and EG will be $\frac{3}{16}$ of an inch.

This is sufficient (though not exact) for giving us a notion of the thing. We see that from this must arise a pressure in the direction DC, viz. the pressure of the whole column HG.

Something of the same kind will happen although the plane AB be wholly immerged, and this even to some depth. We see such elevations in a swift running stream, where there are large stones at the bottom.—This occasions an excess of pressure in the direction opposite to the plane's motion; and we see that there must, in every case, be a relation between the velocity and this excess of pressure. This D'Ulloa expresses by an equation. But it is very exceptionable, not taking properly into the account the comparative facility with which the water can heap up and diffuse itself. It must always heap up till it acquires a sufficient head of water to produce a lateral and progressive diffusion sufficient for the purpose. It is evident, that a smaller elevation will suffice when the body is more immersed, because the check or impulse given by the body below is propagated, not vertically only, but in every direction; and therefore the elevation is not confined to that part of the surface which is immediately above the moving body, but extends so much further laterally as the centre of agitation is deeper: Thus, the elevation necessary for the passage of the body is so much smaller; and it is the *height* only of this accumulation or wave which determines the backward pressure on the body. D'Ulloa's equation may happen to quadrate with two experiments at different depths, without being nearly just; for *any two* points may be in a curve, without exhibiting its equation. Three points will do it with some approach to precision; but four, at least, are necessary for giving any notion of its nature. D'Ulloa has only given two experiments, which we mentioned in another place.

We may here observe, that it is this circumstance which immediately produces the great resistance to the motion of a body through a fluid in a narrow canal.—The fluid cannot pass the body, unless the area of the section be sufficiently extensive. A narrow canal prevents the extension sidewise. The water must therefore heap up, till the section and velocity of diffusion are sufficiently enlarged, and thus a great backward pressure is produced. (See the second series of Experiments by the French Academicians; see also Franklin's Essays).

Thus have we attempted to give our readers some account of one of the most interesting problems in the whole of mechanical philosophy. We are sorry that so little advantage can be derived from the united efforts of the first mathematicians of Europe, and that there is so little hope of greatly improving our scientific knowledge of the subject. What we have delivered will, however, enable our readers to peruse the writings of those who have applied the theories to practical purposes. Such, for instance, are the treatises of John Bernoulli, of Bougner, and of Euler, on the construction and working of ships, and the occasional dissertations of different authors on water-mills. In this last application the ordinary theory is not without its value, for the impulses are nearly perpendicular; in which case they do not materially deviate from the duplicate proportion of the sine of incidence. But even here this theory, applied as it commonly is, misleads us exceedingly. The impulse on one float may be accurately enough stated by it; but the authors have not been attentive to the motion of the water after it has made its impulse; and the impulse on the next float is stated the same as if the parallel filaments of water, which were not stopped by the preceding float, did impinge on the opposite part of the second, in the same manner, and with the same obliquity and energy, as if it were detached from the rest. But this does not in the least resemble the real process of nature.

Suppose the floats B, C, D, H (Fig. 24.) of a wheel immersed in a stream whose surface moves in the direction

AK, and that this surface meets the float B in E. The part BE alone is supposed to be impelled; whereas the water, checked by the float, heaps up on it to *e*.—Then drawing the horizontal line BF, the part CF of the next float is supposed to be all that is impelled by the parallel filaments of the stream; whereas the water bends round the lower edge of the float B by the surrounding pressure, and rises on the float *c* all the way to *f*. In like manner, the float D, instead of receiving an impulse on the very small portion DG, is impelled all the way from D to *g*, not much below the surface of the stream. The surfaces impelled at once, therefore, greatly exceed what this slovenly application of the theory supposes, and the whole impulse is much greater; but this is a fault in the application, and not in the theory.

RIVER,

Is a current of fresh water, flowing in a BED or CHANNEL, from its source to the sea.

1. The term is appropriated to a *considerable* collection of waters, formed by the conflux of two or more BROOKS, which deliver into its channel the united streams of several RIVULETS, which have collected the supplies of many RILLS trickling down from numberless springs, and the torrents which carry off from the sloping grounds the surplus of every shower.

2. Rivers form one of the chief features of the surface of this globe, serving as voiders of all that is immediately redundant in our rains and springs, and also as boundaries and barriers, and even as highways, and in many countries, as plentiful storehouses. They also fertilize our soil by laying upon our warm fields the richest mould, brought from the high mountains, where it would have remained useless for want of genial heat.

3. Being such interesting objects of attention, every branch acquires a proper name, and the whole acquires a sort of personal identity, of which it is frequently difficult to find the principle; for the name of the great body of waters which discharges itself into the sea is traced backwards to one of the sources, while all the contributing streams are lost, although their waters form the chief part of the collection. And sometimes the feeder in which the name is preserved is smaller than others which are united to the current, and which, like a rich but ignoble alliance, lose their name in that of the more illustrious family. Some rivers indeed are respectable even at their birth, coming at

ence is force from some great lake. Such is the Rio de la Plata, the river St Lawrence, and the mighty streams which issue in all directions from the Baical lake. But, like the sons of Adam, they are all of equal descent, and should take their name from one of the feeders of these lakes. This is indeed the case with a few, such as the Rhone, the Rhine, the Nile. These, after having mixed their waters with those of the lake, resume their appearance and their name at its outset.

4. But in general their origin and progress, and even the features of their character, bear some resemblance (as has been prettily observed by Pliny) to the life of man. The river springs from the earth; but its origin is in heaven. Its beginnings are insignificant, and its infancy is frivolous; it plays among the flowers of a meadow; it waters a garden, or turns a little mill. Gathering strength in its youth, it becomes wild and impetuous. Impatient of the restraints which it still meets with in the hollows among the mountains, it is restless and fretful; quick in its turnings, and unsteady in its course. Now it is a roaring cataract, tearing up and overturning whatever opposes its progress, and it shoots headlong down from a rock; then it becomes a sullen and gloomy pool, buried in the bottom of a glen. Recovering breath by repose, it again dashes along, till, tired of the uproar and mischief, it quits all that it has swept along, and leaves the opening of the valley strewed with the rejected waste. Now, quitting its retirement, it comes abroad into the world, journeying with more prudence and discretion through cultivated fields, yielding to circumstances, and winding round what would trouble it to overwhelm or remove. It passes through the populous cities and all the busy haunts of man, tendering its services on every side, and becomes the support and ornament of the country. Now increased by numerous alliances, and advanced in its course of existence, it becomes grave and stately in its motions, loves peace and quiet; and

in majestic silence rolls on its mighty waters, till it is laid to rest in the vast abyss.

The philosopher, the real lover of wisdom, sees much to admire in the economy and mechanism of running waters; and there are few operations of nature which give him more opportunities of remarking the nice adjustment of the most simple means for attaining many purposes of most extensive beneficence. All mankind seem to have felt this. The heart of man is ever open (unless perverted by the habits of selfish indulgence and arrogant self-conceit) to impressions of gratitude and love. He who ascribes the religious principle (debased though it be by the humbling abuses of superstition) to the workings of fear alone, may betray the slavish meanness of his own mind, but gives a very unfair and a false picture of the hearts of his neighbours. Lucretius was but half a philosopher when he penned his often-quoted apothegm. Indeed his own invocations show how much the animal was blended with the sage.

We apprehend, that whoever will read with an honest and candid mind, unbiassed by licentious wishes, the accounts of the ancient superstitions, will acknowledge that the amiable emotions of the human soul have had their share in creating the numerous divinities whose worship filled up their calendars. The sun and the host of heaven have in all ages and nations been the objects of a sincere worship. Next to them, the rivers seem to have attracted the grateful acknowledgments of the inhabitants of the adjacent countries. They have everywhere been considered a sort of tutelar divinities; and each little district, every retired valley, had its river god, who was preferred to all others with a partial fondness. The expostulation of Naaman the Syrian, who was offended with the prophet for enjoining him to wash in the river Jordan, was the natural effusion of this attachment. "What (said he), are not Abana and Pharphat, rivers of Damascus, more excellent than all the waters of Judæa? Might I not wash in them and be clean? So he went away wroth."

In those countries particularly, where the rural labours, and the hopes of the shepherd and the husbandman, were not so immediately connected with the approach and recess of the sun, and depended rather on what happened in a far distant country by the falls of periodical rains or the melting of collected snows, the Nile, the Ganges, the Indus, the river of Pegu, were the sensible agents of nature in procuring to the inhabitants of their fertile banks all their abundance, and they became the objects of grateful veneration. Their sources were sought out with anxious care even by conquering princes; and when found, were universally worshipped with the most affectionate devotion. These remarkable rivers, so eminently and so palpably beneficent, preserve to this day, amidst every change of habit, and every increase of civilization and improvement, the fond adoration of the inhabitants of those fruitful countries through which they hold their stately course, and their waters are still held sacred. No progress of artificial refinement, not all the corruption of luxurious sensuality, has been able to eradicate this plant of native growth from the heart of man. The sentiment is congenial to his nature, and therefore it is universal; and we could almost appeal to the feelings of every reader, whether he does not perceive it in his own breast. Perhaps we may be mistaken in our opinion in the case of the corrupted inhabitants of the populous and busy cities, who are habituated to the fond contemplation of their own individual exertions as the sources of all their hopes. Give the shoemaker but leather and a few tools, and he defies the powers of nature to disappoint him; but the simpler inhabitants of the country, the most worthy and the most respectable part of every nation, after equal, perhaps greater exertion both of skill and of industry, are more accustomed to resign themselves to the great ministers of Providence, and to look up to heaven for the "early and the latter rains," without which all their labours are fruitless.

> ——————— *extrema per illos*
> *Numenque excedens terris vestigia fecit.*

And among the husbandmen and the shepherds of all nations and ages, we find the same fond attachment to their springs and rivulets.

> *Fortunate senex, hic inter flumina nota*
> *Et fontes sacros frigus captabis opacum,*

was the mournful ejaculation of poor Melibœus. We hardly know a river of any note in our own country whose source is not looked on with some respect.

We repeat our assertion, that this worship was the offspring of affection and gratitude, and that it is giving a very unfair and false picture of the human mind to ascribe these superstitions to the working of fear alone. These would have represented the river-gods as seated on ruins, brandishing rooted-up trees, with angry looks, pouring out their sweeping torrents. But no such thing. The lively imagination of the Greeks felt, and expressed with an energy unknown to all other nations, every emotion of the human soul. They figured the Naiads as beautiful nymphs, patterns of gentleness and of elegance. They are represented as partially attached to the children of men; and their interference in human affairs is always in acts of kind assistance and protection. They resemble, in this respect, the rural deities of the northern nations, the fairies, but without their caprices and resentments. And, if we attend to the descriptions and representations of their RIVER-GODS, beings armed with power, an attribute which slavish fear never fails to couple with cruelty and vengeance, we find the same expression of affectionate trust and confidence in their kind dispositions. They are generally called by the respectable but endearing name of *father*. "*Da Tyberi* *pater*," says Virgil. Mr Bruce says that the Nile at its source is called the *abay* or "father."—We observe this word, or its radix, blended with many names of rivers of the east; and think it probable, that when our traveller got this name from the inhabitants of the neighbourhood, they applied to the stream what is meant to express the tutelar or presiding spirit. The river-gods are always represented as venerable old men, to indicate their being coeval with the world. But it is always a *cruda viridisque senectus*, and they are never represented as oppressed with age and decrepitude. Their beards are long and flowing, their looks placid, their attitude easy, reclined on a bank, covered as they are crowned, with never-fading sedges and bulrushes, and leaning on their urns, from which they pour out their plentiful and fertilizing streams.—Mr Bruce's description of the sources of the Nile, and of the respect paid to the sacred waters, has not a frowning feature; and the hospitable old man, with his fair daughter Irepone, and the gentle priesthood which peopled the little village of Geesh, forms a contrast with the neighbouring Galla (among whom a military leader was called the *lamb*, because he did not murder pregnant women), which very distinctly paints the inspiring principle of this superstition. Pliny says (viii. 8.) that at the source of the Clitumnus there is an ancient temple highly respected. The presence and the power of the Divinity are expressed by the fates which stand in the vestibule. Around this temple are several little chapels, each of which covers a sacred fountain; for the Clitumnus is the father of several little rivers which unite their streams with him. At some distance below the temple is a bridge which divides the sacred waters from those which are open to common use. No one must presume to set his foot in the streams above this bridge; and to step over any of them is an indignity which renders a person infamous. They can only be visited in a consecrated boat. Below the bridge we are permitted to bathe, and the place is incessantly occupied by the neighbouring villagers. (See also *Vibius Sequestr. Orbelini*, p. 101—103, and 221—223; also *Sueton. Caligula*, c. 43; *Virg. Georg. II.* 146.)

What is the cause of all this? The Clitumnus flows (near its source) through the richest pastures, through which it was carefully distributed by numberless drains; and these nourished cattle of such spotless whiteness and extraordinary beauty, that they were sought for with eagerness over all Italy, as the most acceptable victims in their sacrifices. Is not this superstition then an effusion of gratitude?

Such are the dictates of kind-hearted nature in our breasts, before it has been vitiated by vanity and self-conceit, and we should not be ashamed of feeling the impression. We hardly think of making any apology for dwelling a little on this incidental circumstance of the superstitious veneration paid to rivers. We cannot think that our readers will be displeased at having agreeable ideas excited in their minds, being always of opinion that the torch of true philosophy will not only enlighten the understanding, but also warm and cherish the affections of the heart.

With respect to the origin of rivers, we have very little to offer in this place. It is obvious to every person, that besides the torrents which carry down into the rivers what part of the rains and melted snows is not absorbed by the soil, or taken up by the plants which cover the earth, they are fed either immediately or remotely by the springs. A few remarkable streams rush at once out of the earth in force, and must be considered as the continuation of subterraneous rivers, whose origin we are therefore to seek out; and we do not know any circumstance in which their first beginnings differ from those of other rivers, which are formed by the union of little streams and rills, each of which has its own source in a spring or fountain. This question, therefore, What is the process of nature, and what are the supplies which fill our springs? will be treated of under the word SPRING.

Whatever be the source of rivers, it is to be met with in almost every part of the globe. The crust of earth with which the rocky framing of this globe is covered is generally stratified. Some of these strata are extremely pervious to water, having but small attraction for its particles, and being very porous. Such is the quality of gravelly strata in an eminent degree. Other strata are much more firm, or attract water more strongly, and refuse it a passage. This is the case with firm rock and with clay. When a stratum of the first kind has one of the other immediately under it, the water remains in the upper stratum, and bursts out wherever the sloping sides of the hills cut off the strata, and this will be in the form of a trickling spring, because the water in the porous stratum is greatly obstructed in its passage towards the outlet. As this irregular formation of the earth is very general, we must have springs, and of course rivers or rivulets, in every corner where there are high grounds.

Rivers flow from the higher to the low grounds. It is the arrangement of this elevation which distributes them over the surface of the earth. And this appears to be accomplished with considerable regularity; and, except the great desert of Kobi, on the confines of Chinese Tartary, we do not remember any very extensive tract of ground that is deprived of those channels for voiding the superfluous waters; and even there they are far from being redundant.

The course of rivers give us the best general method for judging of the elevation of a country. Thus it appears that Savoy and Switzerland are the highest grounds of Europe, from whence the ground slopes in every direction. From the Alps proceed the Danube and the Rhine, whose courses mark the two great valleys, into which many lateral streams descend. The Po also and the Rhone come from the same head, and with a steeper and shorter course find their way to the sea through valleys of less breadth and length. On the west side of the valleys of the Rhine and the Rhone the ground rises pretty fast, so that few tributary streams come into them from that side; and from this gentle elevation France slopes to the westward. If a line nearly straight, but bending a little to the northward, be drawn from the head of Savoy and Switzerland all the way to Solikamskoy in Siberia, it will nearly pass through the most elevated part of Europe; for in this tract most of the rivers have their rise. On the left go off the various feeders of the Elbe, the Oder, the Wesel, the Niemen, the Duna, the Neva, the Dwina, the Petzora. On the right, after passing the feeders of the Danube, we see the sources of the Sereth and Pruth, the Dniester, the Bog, the Dnieper, the Don, and the mighty Volga. The elevation, however, is extremely moderate; and it appears from the levels taken with the barometer by the Abbé Chappe d'Auteroche, that the head of the Volga is not more than 470 feet above the surface of the ocean. And we may observe here by the bye, that its mouth, where it discharges its waters into the Caspian Sea, is undoubtedly lower, by many feet, than the surface of the ocean. See Pneumatics, vol. iii. Spain and Finland, with Lapland, Norway, and Sweden, form two detached parts, which have little symmetry with the rest of Europe.

A chain of mountains begins in Nova Zembla, and stretches due south to near the Caspian Sea, dividing Europe from Asia. About three or four degrees north of the Caspian Sea, it bends to the south-east, traverses western Tartary, and passing between the Tengis and Zaizan lakes, it then branches to the east and south. The eastern branch runs to the shores of Korea and Kamtschatka. The southern branch traverses Turkestan and Thibet, separating them from India, and at the head of the kingdom of Ava joins an arm stretching from the great eastern branch, and here forms the centre of a very singular radiation. Chains of mountains issue from it in every direction. Three or four of them keep very close together, dividing the continent into narrow slips, which have each a great river flowing in the middle, and reaching to the extreme points of Malacca, Cambodia, and Cochin-china; From the same central point proceeds another great ridge due east, and passes a little north of Canton in China. We called this a singular centre: for though it sends off so many branches, it is by no means the most elevated part of the continent. In the triangle which is included between the first southern ridge (which comes from between the lakes Tanges and Zaizan), the great eastern ridge, and its branch, which almost unites with the southern ridge, lies the Boutan, and part of Tibet, and the many little rivers which occupy its surface flow southward and eastward, uniting a little to the north of the centre often mentioned, and then pass through a gorge eastward into China. And it is farther to be observed, that these great ridges do not appear to be seated on the highest parts of the country; for the rivers which correspond to them are at no great distance from them, and receive their chief supplies from the other sides. This is remarkably the case with the great Oby, which runs almost parallel to the ridge from the lakes to Nova Zembla. It receives its supplies from the east, and indeed it has its source far east. The highest grounds (if we except the ridges of mountains which are boundaries) of the continent seem to be in the country of the Calmucs, about 95° east from London, and latitude 43° or 45° north. It is represented as a fine though sandy country, having many little rivers which lose themselves in the sand, or end in little salt lakes. This elevation stretches north-east to a great distance; and in this tract we find the heads of the Irtish, Selenga, and Tunguskaia (the great feeders of the Oby), the Olenitz, the Lena, the Yana, and some other rivers which all go off to the north. On the other side we have the great river Amur, and many smaller rivers, whose names are not familiar. The Hoangho, the great river of China, rises on the south side of the great eastern ridge we have so often mentioned. This elevation, which is a continuation of the former, is somewhat of the same complexion, being very sandy, and at present is a desert of prodigious extent. It is described, however, as interspersed with vast tracts of rich pasture; and we know that it was formerly the residence of a great nation, who came south by the name of *Turks*, and possessed themselves of most of the richest kingdoms of Asia. In the south-western extremity of this country are found remains not only of barbaric magnificence, but even of cultivation and elegance. It was a profitable privilege granted by Peter the Great to some adventurers to search these sandy deserts for remains of former opulence, and many pieces of delicate workmanship, (though not in a style which we would admire) in gold and silver, were found. Vaults were found buried in the sand filled with written papers, in a character wholly unknown; and a wall was discovered extending several miles, built with hewn stone, and ornamented with corniche and battlements. But we are forgetting ourselves, and return to the consideration of the distribution of the rivers on the surface of the earth. A great ridge of mountains begins at the south-east corner of the Euxine Sea, and proceeds eastward, ranging along the south side of the Caspian, and still advancing, unites with the mountains first mentioned in Thibet, sending off some branches to the south, which divide Persia, India, and Thibet. From the south side of this ridge flow the Euphrates, Tigris, Indus, Ganges, &c. and from the north the ancient Oxus and many unknown streams.

There is a remarkable circumstance in this quarter of the globe. Although it seems to be nearest to the greatest elevations, it seems also to have places of the greatest depression. We have already said that the Caspian Sea is lower than the ocean. There is in its neighbourhood another great basin of salt water, the lake Aral, which receives the waters of the Oxus or Gihon, which were said to have formerly run into the Caspian Sea. There cannot therefore be a great difference in the level of these two basins; neither have they any outlet, though they receive great rivers. There is another great lake in the very middle of Persia, the Zare or Zara, which receives the river Hindemend, of near 250 miles in length, besides other streams. There is another such in Asia Minor. The sea of Sodom and Gomorrah is another instance. And in the high countries we mentioned, there are many small salt lakes, which receive little rivers, and have no outlet. The lake Zara in Persia, however, is the only one which indicates a considerable hollow of the country. It is now ascertained, by actual survey, that the Sea of Sodom is considerably higher than the Mediterranean. This feature is not, however, peculiar to Asia. It obtains also in Africa, whose rivers we now proceed to mention.

Of them, however, we know very little. The Nile indeed is perhaps better known than any river out of Europe. By the register of the weather kept by Mr Bruce at Gondar in 1770 and 1771, it appears that the greatest rains are about the beginning of July. He says, that at an average each month after June it doubles its rains. The calish or canal is opened at Cairo about the 9th of August, when the river has risen 14 peeks (each 21 inches), and the waters begin to decrease about the 10th of September. Hence we may form a conjecture concerning the time which the water employs in coming from Abyssinia. Mr Bruce supposes it nine days, which supposes a velocity not less than 14 feet in a second; a thing past belief, and inconsistent with all our notions. The general slope of the river is greatly diminished by several great cataracts; and Mr Bruce expressly says, that he might have come down from Sennar to the cataracts of Syene in a boat, and that it is navigable for boats far above Sennaar. He came from Syene to Cairo by water. We apprehend that no boat would venture down a stream moving even six feet in a second, and none could row up if the velocity was three feet. As the waters begin to decrease about the 10th of September, we must conclude that the water then flowing past Cairo had left Abyssinia when the rains had greatly abated. Judging in this way, we must still allow the stream a velocity of more than six feet. Had the first swell at Cairo been noticed in 1770 or 1771, we might have guessed better. The year that Thevenot was in Egypt, the first swell of 8 peeks was observed January 28. The calish was opened for 14 peeks on August 14th, and the waters began to decrease on September 23d, having risen to 21⅔ peeks. We may suppose a similar progress at Cairo corresponding to Mr Bruce's observations at Gondar, and date every thing five days earlier.

We understand that some of our gentlemen stationed far up the Ganges have had the curiosity to take notes of the swellings of that river, and compare them with the overflowings at Calcutta, and that their observations are about to be made public. Such accounts are valuable additions to our practical knowledge, and we shall not neglect to insert the information in some kindred article of this work.

The same mountains which attract the tropical vapours, and produce the fertilising inundations of the Nile, perform the same office to the famous Niger, whose existence has often been accounted fabulous, and with whose course we have very little acquaintance. The researches of the gentlemen of the African association render its existence no longer doubtful, and have greatly excited the public curiosity. For a farther account of its track, see NIGER.

From the great number, and the very moderate size, of the rivers which fall into the Atlantic Ocean all the way south of the Gambia, we conclude that the western slope is the most elevated, and that the mountains are at no great distance inland. On the other hand, the rivers of Melinda and Sofala are of a magnitude which indicates a much longer course. But of all this we speak with much uncertainty.

The frame-work (so to call it) of America is better known, and is singular.

A chain of mountains begins, or at least is found, in longitude 110° west of London, and latitude 40° north, on the northern confines of the kingdom of Mexico, and stretching southward through that kingdom, forms the ridge of the neck of land which separates North from South America, and keeping almost close to the shore, ranges along the whole western coast of South America, terminating at Cape Horn. In its course it sends off branches, which, after separating from it for a few leagues, rejoin it again, enclosing valleys of great extent from north to south, and of prodigious elevation. In one of these, under the equatorial sun, stands the city of Quito, in the midst of extensive fields of barley, oats, wheat, and gardens, containing apples, pears, and gooseberries, and, in short, all the grains and fruits of the cooler parts of Europe; and although the vine is also there in perfection, the olive is wanting. Not a dozen miles from it in the low countries, the sugar-cane, the indigo, and all the fruits of the torrid zone, find their congenial heat, and the inhabitants swelter under a burning sun. At as small a distance on the other hand, tower aloft the pinnacles of Pichincha, Corambourou, and Chimboracô, crowned with never-melting snows.

The individual mountains of this stupendous range not only exceed in height all others in the world (if we except the Peak of Teneriffe, Mount Ætna, and Mount Blanc), but they are set down on a base incomparably more elevated than any other country. They cut off therefore all communication between the Pacific Ocean and the inland continent; and no rivers are to be found on the west coast of South America which have any considerable length of course or body of waters. The country is drained like Africa, in the opposite direction. Not 100 miles from the city of Lima, the capital of Peru, which lies almost on the sea shore, and just at the foot of the high Cordilleras, arises out of a small lake the Maragnon or Amazon's river, which, after running northward for about 100 miles, takes an easterly direction, and crosses nearly the broadest part of South America, and falls into the great western ocean at Para, after a course of not less than 3500 miles. In the first half of its descent it receives a few middle-sized rivers from the north, and from the south it receives the great river Combos, springing from another little lake not 50 miles distant from the head of the Maragnon, and enclosing between them a wide extent of country. Then it receives the Yuta, the Yuerva, the Cuchivara, and Parana Mire, each of which is equal to the Rhine; and then the Madeira, which has flowed above 1300 miles. At their junction the breadth is so great, that neither shore can be seen by a person standing up in a canoe; so that the united stream must be about six miles broad. In this majestic form it rolls along at a prodigious rate through a flat country, covered with impenetrable forests, and most of it as yet untrodden by human feet. Mr Condamine, who came down the stream, says, that all is silent as the desert, and the wild beasts and numberless birds crowd round the boat, eyeing it as some animal of which they did not seem afraid. The bed was cut deep through an equal and yielding soil, which seemed rich in every part, if he could judge by the vegetation, which was rank in the extreme. What an addition this to the possible population of this globe! A narrow slip along each bank of this mighty river would equal in surface the whole of Europe, and would probably exceed it in general fertility: and although the velocity in the main stream was great, he observed that it was extremely moderate, nay almost still, at the sides; so that in those parts where the country was inhabited by men, the Indians paddled up the river with perfect ease. Boats could go from Para to near the mouth of the Madeira in 38 days, which is near [illegible] miles.

Mr Condamine made an observation during his passage down the Maragnon, which is extremely curious and instructive, although it puzzled him very much. He observed that the tide was sensible at a vast distance from the mouth: it was very considerable at the junction of the Madeira; and he supposes that it might have been observed much farther up. This appeared to him very surprising, because there could be no doubt but that the surface of the water there was higher by a great many feet than the surface of the flood of the Atlantic Ocean at the mouth of the river. It was therefore very natural for him to ascribe the tide in the Maragnon to the immediate action of the moon on its waters; and this explanation was the more reasonable, because the river extends in the direction of terrestrial longitude, which, by the Newtonian theory, is most favourable to the production of a tide. Journeying as he did in an Indian canoe, we cannot suppose that he had much leisure or conveniency for calculations, and therefore are not surprised that he did not see that even this circumstance was of little avail in so small or shallow a body of water. He carefully noted, however, the times of high and low water as he passed along. When arrived at Para, he found not only that the high water was later and later as we are farther from the mouth, but he found that at one and the same instant there were several points of high water between Para and the confluence of the Madeira, with points of low water intervening. This conclusion was easily drawn from his own observations, although he could not see at one instant the high waters in different places. He had only to compute the time of high water at a particular spot, on the day he observed it at another; allowing, as usual, for the moon's change of position. The result of his observations therefore was, that the surface of the river was not an inclined plane whose slope was lessened by the tide of flood at the mouth of the river, but that it was a waving line, and that the propagation of the tide up the river was nothing different from the propagation of any other wave. We may conceive it clearly, though imperfectly, in this way. Let the place be noted where the tide happens 12 hours later than at the mouth of the river. It is evident that there is also a tide at the very mouth at the same instant; and, since the ocean tide had withdrawn itself during the time that the former tide had proceeded so far up the river, and the tide of ebb is successively felt above as well as the tide of flood, there must be a low water between these two high waters.

Newton had pointed out this curious fact, and observed that the tide at London-Bridge, which is 43 feet above the sea, is not the same with that at Gravesend, but the preceding tide (See *Phil. Trans.* 67.) This will be more particularly insisted on in another place.

Not far from the head of the Maragnon, the Cordilleras send off a branch to the north-east, which reaches and ranges along the shore of the Mexican Gulf, and the Rio Grande de Sta Martha occupies the angle between the ridges.

Another ridge ranges with interruptions along the east coast of Terra Firma, so that the whole waters of this country are collected into the Oroonoko. In like manner the north and east of Brasil are hemmed in by mountainous ridges, through which there is no considerable passage; and the ground sloping backwards, all the waters of this immense track are collected from both sides by many considerable rivers into the great river Paraguay, or Rio de la Plata, which runs down the middle of this country for more than 1400 miles, and falls into the sea through a vast [illegible] in latitude 35°.

Thus the whole of South America seems as if it had formerly surrounded by a mound, and been a great lake. The ground in the middle, where the Parama, the Madeira and the Plata, take their rise, is an immense marsh uninhabitable for its exhalations, and quite impervious in its present state.

The manner in which the continent of North America is watered, or rather drained, has also some peculiarities. By looking at the map, one will observe first of all a general division of the whole of the best known part into two by the valleys in which the beds of the river St Laurence and Mississippi are situated. The head of this is occupied by a singular series of fresh-water seas or lakes, viz. the lake Superior and Michigan, which empty themselves into lake Huron by two cataracts. This again runs into lake Erie by the river Detroit, and the Erie pours its waters into the Ontario by the famous fall of Niagara, and from the Ontario proceeds the great river St Laurence.

The ground to the south-west of the lakes Superior and Erie is somewhat lower, and the middle of the valley is occupied by the Mississippi and the Missouri, which receive on both sides a number of smaller streams, and having joined, proceed to the south, under the name Mississippi. In latitude 37, this river receives into its bed the Ohio, a river of equal magnitude, and the Cherokee river, which drains all the country lying at the back of the United States, separated from them by the ranges of the Apalachian mountains. The Mississippi is now one of the chief rivers on the globe, and proceeds due south, till it falls into the Mexican bay through several shifting mouths, which greatly resemble those of the Danube and the Nile, having run above 1200 miles.

The elevated country between this bed of the Mississippi and St Laurence and the Atlantic Ocean is drained on the east side by a great number of rivers, some of which are very considerable, and of long course; because, instead of being nearly at right angles to the coast, as in other countries, they are in a great measure parallel to it. This is more remarkably the case with Hudson's river, the Delaware, Patomack, Rapahanoc, &c. Indeed the whole of North America seems to consist of ribs or beams laid nearly parallel to each other from north to south, and the rivers occupy the interstices. All those which empty themselves into the bay of Mexico are parallel and almost perfectly straight, unlike what are seen in other parts of the world. The westermost of them all, the North River, as it is named by the Spaniards, is nearly as long as the Mississippi.

We are very little informed as yet of the distribution of rivers on the north-west coast of America, or the course of those which run into Hudson's and Baffin's Bay.

The Maragnon is undoubtedly the greatest river in the world, both as to length of run and the vast body of water which it rolls along. The other great rivers succeed nearly in the following order:

Maragnon,
Senegal,
Nile,
St Laurence,
Hoangho,
Rio de la Plata,
Yenisey,
Mississippi,
Volga,
Oby,
Amur,
Oroonoko,
Ganges,
Euphrates,
Danube,
Don,
Indus,
Dnieper,
Duina,
&c.

We have been much assisted in this account of the course of rivers, and their distribution over the globe, by a beautiful planisphere, or map of the world, published by Mr Bode, astronomer royal at Berlin. The ranges of mountains are there laid down with philosophical discernment and precision; and we recommend it to the notice of our geographers. We cannot divine what has caused Mr Buffon to say that the course of most rivers is from east to west, or from west to east. No physical point of his system seems to require it, and it needs only that we look at his own map to see its falsity. We should naturally expect to find the *general* course of rivers nearly perpendicular to the line of sea-coast; and we find it so; and the chief exceptions are in opposition to Mr Buffon's assertion. The structure of America is so particular, that *very few* of its rivers have their general course in this direction. We proceed now to consider the motion of rivers; a subject which naturally resolves itself into two parts, *theoretical* and *practical*.

PART I.

THEORY OF THE MOTION OF RIVERS AND CANALS.

THE importance of this subject needs no commentary. Every nation, every country, every city, is interested in it. Neither our wants, our comforts, nor our pleasures, can dispense with an ignorance of it. We must conduct their waters to the centre of our dwellings; we must secure ourselves against their ravages; we must employ them to drive those machines which, by compensating for our personal weakness, make a few able to perform the work of thousands; we employ them to water and fertilize our fields, to decorate our mansions, to cleanse and embellish our cities, to preserve or extend our demesnes, to transport from county to county every thing which necessity, convenience, or luxury, has rendered precious to man:

for these purposes we must confine and govern the mighty rivers, we must preserve or change the beds of the smaller streams, draw off from them what shall water our fields, drive our machines, or supply our houses. We must keep up their waters for the purposes of navigation, or supply their places by canals; we must drain our fens, and defend them when drained; we must understand their motions, and their mode of secret, slow, but unceasing action, that our bridges, our wharfs, our dikes, may not become heaps of ruins. Ignorant how to proceed in these daily-recurring cases, how often do we see projects of high expectation and heavy expense fail of their object, leaving the state burdened with works not only useless but frequently hurtful?

This has long been a most interesting subject of study in Italy, where the fertility of their fields is not more indebted to their rich soil and happy climate, than to their numerous derivations from the rivers which traverse them: and in Holland and Flanders, where their very existence requires unceasing attention to the waters, which are every moment ready to swallow up the inhabitants, and where the inhabitants, having once subdued this formidable enemy, have made those very waters their indefatigable drudges, transporting through every corner of the country the materials of the most extensive commerce on the face of this globe.

Such having been our incessant occupations with moving waters, we should expect that while the operative artists are continually furnishing facts and experiments, the man of speculative and scientific curiosity, excited by the importance of the subject, would ere now have made considerable progress in the science; and that the professional engineer would be daily acting from established principle, and be seldom disappointed in his expectations. Unfortunately the reverse of this is nearly the true state of the case; each engineer is obliged to collect the greatest part of his knowledge from his own experience, and by many dear-bought lessons, to direct his future operations, in which he still proceeds with anxiety and hesitation: for we have not yet acquired principles of theory, and experiments have not yet been collected and published, by which an empirical practice might be safely formed. Many experiments of inestimable value are daily made; but they remain with their authors, who seldom have either leisure, ability, or generosity, to add them to the public stock.

The motion of waters have been really so little investigated as yet, that hydraulics may still be called a new study. We have merely skimmed over a few common notions concerning the motions of water; and the mathematicians of the first order seem to have contented themselves with such views as allowed them to entertain themselves with elegant applications of calculus. This, however, has not been their fault. They rarely had any opportunity of doing more, for want of a knowledge of facts. They have made excellent use of the few which have been given them; but it required much labour, great variety of opportunity, and great expense, to learn the multiplicity of things which are combined even in the simplest cases of water in motion. These are seldom the lot of the mathematician; and he is without blame when he enjoys the pleasures within his reach, and cultivates the science of geometry in its most abstracted form. Here he makes a progress which is the boast of human reason, being almost ensured from error by the intellectual simplicity of his subject. But when we turn our attention to material objects, and without knowing either the size and shape of the elementary particles, or the laws which nature has prescribed for their action, presume to foresee their effects, calculate their exertions, direct their actions, what must be the consequence? Nature shows her independence with respect to our notions, and, always faithful to the laws which are enjoined, and of which we are ignorant, she never fails to thwart our views, to disconcert our projects, and render useless all our efforts.

To wish to know the nature of the elements is vain, and our gross organs are insufficient for the study. To suppose what we do not know, and to fancy shapes and sizes at will; this is to raise phantoms, and will produce a system, but will not prove a foundation, for any science. But to interrogate Nature herself, study the laws which she so faithfully observes, catch her, as we say, in the fact, and thus wrest from her the secret; this is the only way to become her master, and it is the only procedure consistent with good sense. And we see, that soon after Kepler detected the laws of the planetary motions, when Galileo discovered the uniform acceleration of gravity, when Paschal discovered the pressure of the atmosphere, and Newton discovered the laws of attraction and the track of a ray of light; astronomy, mechanics, hydrostatics, chemistry, optics, quickly became bodies of sound doctrine; and the deductions from their respective theories were found fair representations of the phenomena of nature. Whenever a man has discovered a law of nature, he has laid the foundation of a science, and he has given us a new mean of subjecting to our service some element hitherto independent: and so long as groups of natural operations follow a route which appears to us whimsical, and will not admit our calculations, we may be assured that we are ignorant of the principle which connects them all, and regulates their procedure.

This is remarkably the case with several phenomena in the motions of fluids, and particularly in the motion of water in a bed or conduit of any kind. Although the first geniuses of Europe have for this century past turned much of their attention to this subject, we are almost ignorant of the *general laws* which may be observed in their motions. We have been able to select very few points of resemblance, and every case remains nearly an individual. About 150 years ago we discovered, by experience only, the quantity and velocity of water issuing from a small orifice, and, after much labour, have extended this to any orifice; and this is almost the whole of our confidential knowledge. But as to the uniform course of the streams which water the face of the earth, and the maxims which will certainly regulate this agreeably to our wishes, we are in a manner totally ignorant. Who can pretend to say what is the velocity of a river of which you tell him the breadth, the depth, and the declivity? Who can say what swell will be produced in different parts of its course, if a dam or weir of given dimensions be made in it, or a bridge be thrown across it? or how much its waters will be raised by turning another stream into it, or sunk by taking off a branch to drive a mill? Who can say with confidence what must be the dimensions or slope of this branch, in order to furnish the water that is wanted, or the dimensions and slope of a canal which shall effectually drain a fenny district? Who can say what form will cause or will prevent the undermining of banks, the forming of elbows, the pooling of the bed, or the deposition of sands? Yet these are the most important questions.

The causes of this ignorance are the want or uncertainty of our principles; the falsity of our only theory, which is belied by experience; and the small number of proper observations or experiments, and difficulty of making such as shall be serviceable. We have, it is true, made a few experiments on the efflux of water from small orifices, and from them we have deduced a sort of theory, dependent on the fall of heavy bodies and the laws of hydrostatic pressure. Hydrostatics is indeed founded on very simple principles, which give a very good account of the laws of the quiescent equilibrium of fluids, in consequence of gravity and perfect fluidity. But by what train of reasoning can we connect these with the phenomena of the uniform motion of the waters of a river or open stream, which can derive its motion only from the slope of its surface, and the modifications of this motion or its velocity only from the width and depth of the stream? These are the only circumstances which can distinguish a portion of a river from a vessel of the same size and shape, in which, however, the water is at rest. In both, gravity is the sole cause of pressure and motion; but there must be some circumstance peculiar to running waters, which modifies the exertions of the active principle, and which, when discovered, must be the basis of hydraulics, and must oblige us to reject every theory founded on fancied hypotheses, and which can only lead to absurd conclusions: and surely absurd consequences, when legitimately drawn, are complete evidence of improper principles.

When it was discovered experimentally, that the velocities of water issuing from orifices at various depths under the surface were as the square roots of those depths, and the fact was verified by repeated experiments, this principle was immediately and without modification applied to every motion of water. Mariotte, Varignon, Guglielmini, made it the basis of complete systems of hydraulics, which prevail to this day, after having received various amendments and modifications. The same reasoning obtains through them all, though frequently obscured by other circumstances, which are more conspicuously expressed by Guglielmini in his Fundamental Theorems.

He considers every point P (Plate IX. Fig. 1.) in a mass of fluid as an orifice in the side of a vessel, and conceives the particle as having a tendency to move with the same velocity with which it would issue from the orifice. Therefore, if a vertical line APC be drawn through that point, and if this be made the axis of a parabolic ADE, of which A at the surface of the fluid is the vertex, and AB (four times the height through which a heavy body would fall in a second) is the parameter, the velocity of this particle will be represented by the ordinate PD of this parabola; that is, PD is the space which it would uniformly describe in a second.

From this principle is derived the following theory of running waters:

Let DC (Fig. 2.) be the horizontal bottom of a reservoir, to which is joined a sloping channel CK of uniform breadth, and let AB be the surface of the standing water in the reservoir. Suppose the vertical plane BC pierced with an infinity of holes, through each of which the water issues. The velocity of each filament will be that which is acquired by falling from the surface AB.† The filament C, issuing with this velocity, will then glide down the inclined plane like any other heavy body; and (by the common doctrine of the motion down an inclined plane) when it has arrived at F, it will have the same velocity which it would have acquired by falling through the height OF, the point O being in the horizontal plane AB produced. The same may be said of its velocity when it arrives at H or K. The filament immediately above C will also issue with a velocity which is in the subduplicate ratio of its depth, and will then glide down above the first filament. The same may be affirmed of all the filaments; and of the superficial filament, which will occupy the surface of the descending stream.

From this account of the genesis of a running stream of water, we may fairly draw the following consequences:

1. The velocity of any particle R, in any part of the stream, is that acquired by falling from the horizontal plane AN.

2. The velocity at the bottom of the stream is everywhere greater than anywhere above it, and is least of all at the surface.

3. The velocity of the stream increases continually as the stream recedes from its source.

4. The depths EF, GH, &c. in different parts of the stream, will be nearly in the inverse subduplicate ratio of the depths under the surface AN: for since the same quantity of water is running through every section EF and

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† See *Guglielmini's Hydraulics.*

GH, and the channel is supposed of uniform breadth, the depth of each section must be inversely as the velocity of the water passing through it. This velocity is indeed different in different filaments of the section; but the mean velocity in each section is in the subduplicate ratio of the depth of the filament under the surface AB. Therefore the stream becomes more shallow as it recedes from the source; and in consequence of this the difference between LH and MG continually diminishes, and the velocities at the bottom and surface of the stream continually approach to equality, and at a great distance from the source they differ insensibly.

5. If the breadth of the stream be contracted in any part, the depth of the running water will be increased in that part, because the same quantity must still pass through; but the velocity at the bottom will remain the same, and that at the surface will be less than it was before; and the area of the section will be increased on the whole.

6. Should a sluice be put across the stream, dipping a little into the water, the water must immediately rise on the upper side of the sluice till it rises above the level of the reservoir, and the smallest immersion of the sluice will produce this effect. For by lowering the sluice, the area of the section is diminished, and the velocity cannot be increased till the water heap up to a greater height than the surface of the reservoir, and this acquires a pressure which will produce a greater velocity of efflux through the orifice left below the sluice.

7. An additional quantity of water coming into this channel will increase the depth of the stream, and the quantity of water which it conveys; but it will not increase the velocity of the bottom filaments, unless it comes from a higher source.

All these consequences are contrary to experience, and show the imperfection, at least, of the explanation.

The third consequence is of all the most contrary to experience. If any one will but take the trouble of following a single brook from its source to the sea, he will find it most rapid in its beginnings among the mountains, gradually slackening its pace as it winds among the hills and gentler declivities, and at last creeping slowly along through the flat grounds, till it is checked and brought to rest by the tides of the ocean.

Nor is the second consequence more agreeable to observation. It is universally found, that the velocity of the surface in the middle of the stream is the greatest of all, and that it gradually diminishes from thence to the bottom and sides.

And the first consequence, if true, would render the running waters on the surface of this earth the instruments of immediate ruin and devastation. If the waters of our rivers, in the cultivated parts of a country, which are two, three, and four hundred feet lower than their sources, run with the velocity due to that height, they would in a few minutes lay the earth bare to the very bones.

The velocities of our rivers, brooks, and rills, being so greatly inferior to what this theory assigns to them, the other consequences are equally contrary to experience. When a stream has its section diminished by narrowing the channel, the current increases in depth, and this is always accompanied by an increase of velocity through the whole of the section, and most of all at the surface; and the area of the section does not increase, but diminishes, all the phenomena thus contradicting in every circumstance the deduction from the theory; and when the section has been diminished by a sluice let down into the stream, the water gradually heaps up on the upper side of the sluice, and, by its pressure, produces an acceleration of the stream below the sluice, in the same way as if it were the beginning of a stream, as explained in the theory. The velocity now is composed of the velocity preserved from the source and the velocity produced by this subordinate accumulation; and this accumulation and velocity continually increase, till they become such that the whole supply is again discharged through this contracted section: any additional water not only increases the quantity carried along the stream, but also increases the velocity, and therefore the section does not increase in the proportion of the quantity.

It is surprising that a theory really founded on a conceit, and which in every the most familiar and obvious circumstance is contradicted by facts, should have met with so much attention. That Varignon should immediately catch at this notion of Guglielmini, and make it the subject of many elaborate analytical memoirs, is not to be wondered at. This author only wanted *donner prise au calcul;* and it was a usual joke among the academicians of Paris, when any new theorem was invented, *donnons le à Vaignon à generaliser*. But his numerous theorems and corollaries were adopted by all, and still make the substance of the present systems of hydraulics. Gravesande, Mushenbrock, and all the elementary treatises of natural philosophy, deliver no other doctrines; and Belidor, who has been considered as the first of all the scientific engineers, details the same theory in his great work the *Architecture Hydraulique.*

Guglielmini was, however, not altogether the dupe of his own ingenuity. He was not only a pretty good mathematician, but an assiduous and sagacious observer. He had applied his theory to some important cases which occurred in the course of his profession as inspector of the rivers and canals in the Milanese, and to the course of the Danube; and could not but perceive that great corrections were necessary for making the theory quadrate in some tolerable manner with observation; and he immediately saw that the motion was greatly obstructed by inequalities of the canal, which gave to the contiguous filaments of the stream transverse motions, which thwarted and confused the regular progress of the rest of the stream, and thus checked its general progress. These obstructions, he observed, were most effectual in the beginning of its course, while yet a small rill, running among stones, and in a very unequal bed. The whole stream being small, the inequalities bore a great proportion to it, and thus the general effect was great. He also saw that the same causes (these tranverse motions produced by the unequal bottom) chiefly affected the contiguous filaments, and were the reasons why the velocity at the sides and bottom was so much diminished as to be less than the superficial velocity, and that even this might come to be diminished by the same cause. For he observed, that the general stream of a river is frequently composed of a sort of boiling or tumbling motion, by which masses of water are brought up to the surface and again descend. Every person must recollect such appearances in the freshes of a muddy river; and in this way Guglielmini was enabled to account in some measure for the disagreement of his theory with observation.

Mariotte had observed the same obstructions even in the smoothest glass pipes. Here it could not be ascribed to the checks occasioned by tranverse motions. He therefore ascribed it to friction, which he supposed to diminish the motion of fluid bodies in the same manner as of solids: and he thence concludes, that the filaments which immediately rub on the sides of the tube have their velocity gradually diminished; and that the filaments immediately adjoining to these, being thus obliged to pass over them or outstrip them, rub upon them, and have their own velocity diminished in like manner, but in a smaller degree; and that the succeeding filaments towards the axis of the tube suffer similar but smaller diminutions. By this means the whole stream may come to have a smaller velocity; and, at any rate, the medium velocity by which the quantity discharged is determined, is smaller than it would have been independent of friction.

Guglielmini adopted this opinion of Mariotte, and in his next work, on the Motion of Rivers, considered this as the *chief* cause of the retardation; and he added a third circumstance, which he considered as of no less consequence, the viscidity or tenacity of water. He observes that syrup, oil, and other fluids, where this viscidity is more remarkable, have their motions prodigiously retarded by it, and supposes that water differs from them only in the degree in which it possesses this quality: and he says, that by this means not only the particles which are moving more rapidly have their motions diminished by those in their neighbourhood which move slower, but that the filaments also which would have moved more slowly are accelerated by their more active neighbours; and that in this manner the superficial and inferior velocities are brought nearer to an equality. But this will never account for the universal fact, that the superficial particles are the swiftest of all. The superficial particles, says he, acquire by this means a greater velocity than the parabolic law allows them; the medium velocity is often in the middle of the depth; the numerous obstacles, continually multiplied and repeated, cause the current to lose the velocity acquired by the fall; the slope of the bottom then diminishes, and often becomes very small, so that the force remaining is hardly able to overcome the obstacles which are still repeated, and the river is reduced almost to a state of stagnation. He observes, that the Rheno, a river of the Milanese, has near its mouth a slope of no more than 50″, which he considers as quite inadequate to the task; and here he introduces another principle, which he considers as an essential part of the theory of open currents. This is, that there arises from the very depth of the stream a propelling force which restores a part of the lost velocity. He offers nothing in proof of this principle, but uses it to account for and explain the motion of waters in horizontal canals. The principle has been adopted by the numerous Italian writers on hydraulics; and, by various contrivances, interwoven with the parabolic theory, as it is called, of Guglielmini. Our reader may see it in various modifications in the *Idrostatica e Idraulica* of P. Lecchi, and in the *Sperienze Idrauliche* of Michelotti. It is by no means distinct either in its origin or in the manner of its application to the explanation of phenomena, and seems only to serve for giving something like consistency to the vague and obscure discussions which have been published on this subject in Italy. We have already remarked, that in that country the subject is particularly interesting, and has been much commented upon. But the writers of England, France, and Germany, have not paid so much attention to it, and have more generally occupied themselves with the motion of water in close conduits, which seem to admit of a more precise application of mathematical reasoning.

Some of those have considered with more attention the effects of friction and viscidity. Sir Isaac Newton, with his usual penetration, had seen distinctly the manner in which it behoved these circumstances to operate. He had occasion, in his researches into the mechanism of the celestial motions, to examine the famous hypothesis of Descartes, that the planets were carried round the sun by fluid vortices, and saw that there would be no end to uncertainty and dispute till the *modus operandi* of these vortices was mechanically considered. He therefore employed himself in the investigation of the manner in which the acknowledged powers of natural bodies, acting according to the received laws of mechanics, could produce and preserve these vortices, and restore that motion which was expended in carrying the planets round the sun. He therefore, in the second book of the Principles of Natural Philosophy, gives a series of beautiful propositions, viz. 51, 52, &c. with their corollaries, showing how the rotation of a cylinder or sphere round its axis in the midst of a fluid will excite a vortical motion in this fluid; and he ascertains with mathematical precision the motion of every filament of this vortex.

He sets out from the supposition that this motion is excited in the surrounding stratum of fluid in consequence of a want of perfect lubricity, and assumes as an hypothesis, that the initial resistance (or diminution of the motion of the cylinder) which arises from this want of lubricity, is proportional to the velocity with which the surface of the cylinder is separated from the contiguous surface of the surrounding fluid, and that the whole resistance is proportional to the velocity with which the parts of the fluid are mutually separated from each other. From this, and the equality of action and re-action, it evidently follows, that the velocity of any stratum of the vortex is the arithmetical medium between the velocities of the strata immediately within and without it. For the intermediate stratum cannot be in equilibrio, unless it is as much pressed forward by the superior motion of the stratum within it as it is kept back by the slower motion of the stratum without it.

This beautiful investigation applies in the most perfect manner to every change produced in the motion of a fluid filament, in consequence of the viscidity and friction of the adjoining filaments; and a filament proceeding along a tube at some small distance from the sides has, in like manner, a velocity which is the medium between those of the filaments immediately surrounding it. It is therefore a problem of no very difficult solution to assign the law by which the velocity will gradually diminish as the filament recedes from the axis of a cylindrical tube. It is somewhat surprising that so neat a problem has never occupied the attention of the mathematicians during the time that these subjects were so assiduously studied; but so it is, that nothing precise has been published on the subject. The only approach to a discussion of this kind is a Memoir of

Mr Pitot, read to the academy of Paris in 1726, where he considers the velocity of efflux through a pipe. Here, by attending to the comparative superiority of the *quantity of motion* in large pipes, he affirms, that the total diminutions arising from friction will be (*cæteris paribus*) in the inverse ratio of the diameters. This was thankfully received by other writers, and is now a part of our hydraulic theories. It has not, however, been attended to by those who write on the motion of rivers, though it is evident that it is applicable to these with equal propriety; and had it been introduced, it would at once have solved all their difficulties, and particularly would have shown how an almost imperceptible declivity would produce the gentle motion of a great river, without having recourse to the unintelligible principle of Guglielmini.

Mr Couplet made some experiments on the motion of the water in the great main pipes of Versailles, in order to obtain some notions of the retardations occasioned by friction. They were found prodigious; but were so irregular, and unsusceptible of reduction to any general principle, (and the experiments were indeed so few that they were unfit for this reduction), that he could establish no theory.—What Mr Belidor established on them, and makes a sort of system to direct future engineers, is quite unworthy of attention.

Upon the whole, this branch of hydraulics, although of much greater practical importance than the conduct of water in pipes, has never yet obtained more than a vague, and, we may call it, slovenly attention from the mathematicians; and we ascribe it to their not having taken the pains to settle its first principles with the same precision as had been done in the other branch. They were, from the beginning, satisfied with a sort of applicability of mathematical principles, without ever making the application. Were it not that some would accuse us of national partiality, we would ascribe it to this, that Newton had not pointed out the way in this as in the other branch. For any intelligent reader of the performances on the motions of fluids in close vessels, will see that there has not a principle, nay hardly a step of investigation, been added to those which were used or pointed out by Sir Isaac Newton. He has no where touched this question, the motion of water in an open canal. In his theories of the tides, and of the propagation of waves, he had an excellent opportunity for giving at once the fundamental principles of motion in a free fluid whose surface was not horizontal. But, by means of some of those happy and shrewd guesses, in which, as Daniel Bernoulli says, he excelled all men, he saw the undoubted consequences of some palpable phenomenon which would answer all his present purposes, and therefore entered no farther into the investigation.

The original theory of Guglielmini, or the principle adopted by him, that each particle of the vertical section of a running stream has a tendency to move as if it were issuing from an orifice at that depth under the surface, is false; and that it really does so in the face of a dam when the flood-gate is taken away, is no less so; and if it did, the subsequent motions would hardly have any resemblance to those which he assigns them. Were this the case, the exterior form of the cascade would be something like what is sketched in Plate ~~IX.~~ Fig. 3. with an abrupt angle at B, and a concave surface BEG. This will be evident to every one who combines the greater velocity of the lower filaments with the slower motion of those which must slide down above them. But this greater advance of the lower filaments cannot take place without an expenditure of the water under the surface AB. Th surface therefore sinks, and B instantly ceases to retain its place in the horizontal plane. The water does not successively flow forward from A to B, and then tumble over the precipice; but immediately upon opening the flood-h

gate, the water wastes from the space immediately behind it, and the whole puts on the form represented in Fig. 4, consisting of the curve A h P c EG, convex from A to; and concave from thence forward. The superficial water begins to accelerate all the way from A; and the particles may be supposed (for the present) to have acquired the velocity corresponding to their depth under the horizontal surface. This must be understood as nothing more than a vague sketch of the motions. It requires a very critical and intricate investigation to determine either the form of the upper curve or the motions of the different filaments. The place A, where the curvature begins, is of equally difficult determination, and is various according to the differences of depth and of inclination of the succeeding canal.

We have given this sort of history of the progress which had been made in this part of hydraulics, that our readers might form some opinion of the many dissertations which have been written on the motion of rivers, and of the state of the arts depending on it. Much of the business of the civil engineer is intimately connected with it: and we may therefore believe, that since there was so little principle in the theories, there could be but very little certainty in the practical operations. The fact has been, that no engineer could pretend to say, with any precision, what would be the effect of his operations. One whose business had given him many opportunities, and who kept accurate and judicious registers of his own works, could pronounce, with some probability, how much water would be brought off by a drain of certain dimensions and a given slope, when the circumstances of the case happened to tally with some former work in which he had succeeded or failed; but out of the pale of his own experience he could only make a sagacious guess. A remarkable instance of this occurred not long ago. A small aqueduct was lately carried into Paris. It had been conducted on a plan presented to the academy, who had corrected it, and gave a report of what its performance would be. When executed in the most accurate manner, it was deficient in the proportion of five to nine. When the celebrated Desaguliers was employed by the city of Edinburgh to superintend the bringing in the water for the supply of the city, he gave a report on the plan which was to be followed. It was executed to his complete satisfaction; and the quantity of water delivered was about one-sixth of the quantity which he promised, and about one-eleventh of the quantity which the no less celebrated M'Laurin calculated from the same plan.

Such being the state of our theoretical knowledge (if it can be called by this name), naturalists began to be persuaded that it was but losing time to make any use of a theory so incongruous with observation, and that the only safe method of proceeding was to multiply experiments in every variety of circumstances, and to make a series of experiments in every important case, which should comprehend all the practicable modifications of that case. Perhaps circumstances of resemblance might occur, which would enable us to connect many of them together, and at last discover the principles which occasioned this connection; by which means a theory founded on science might be obtained. And if this point should not be gained, we might perhaps find a few general facts, which are modified in all these particular cases, in such a manner that we can still trace the general facts, and see the part of the particular case which depends on it. This would be the acquisition of what may be called an empirical theory, by which every phenomenon would be explained, in so far as the explanation of a phenomenon is nothing more than the pointing out the general fact or law under which it is comprehended; and this theory would answer every practical purpose, because we should confidently foresee what consequences would result from such and such premises; or, if we should fail even in this, we should still have a series of experiments so comprehensive, that we could tell what place in the series would correspond to any particular case which might be proposed.

There are two gentlemen, whose labours in this respect deserve very particular notice, professor Michelotti at Turin, and Abbé Bossut at Paris. The first made a prodigious number of experiments both on the motion of water through pipes and in open canals. They were performed at the expense of the sovereign, and no expense was spared. A tower was built of the finest masonry, to serve as a vessel from which the water was to issue through holes of various sizes, under pressures from 5 to 22 feet. The water was received into basins constructed of masonry and nicely lined with stucco, from whence it was conveyed in canals of brick-work lined with stucco, and of various forms and declivities. The experiments on the expense of water through pipes are of all that have yet been made the most numerous and exact, and may be appealed to on every occasion. Those made in open canals are still more numerous, and are no doubt equally accurate; but they have not been so contrived as to be so generally useful, being in general very unlike the important cases which will occur in practice, and they seem to have been contrived chiefly with the view of establishing or overturning certain points of hydraulic doctrine which were probably prevalent at the time among the practical hydraulists.

The experiments of Bossut are also of both kinds; and though on a much smaller scale than those of Michelotti, seem to deserve equal confidence. As far as they follow the same track, they perfectly coincide in their results, which should procure confidence in the other; and they are made in situations much more analogous to the usual practical cases. This makes them doubly valuable. They are to be found in his two volumes entitled *Hydrodynamique*. He has opened this path of procedure in a manner so new and so judicious, that he has in some measure the merit of such as shall follow him in the same path.

This has been most candidly and liberally allowed him by the chevalier de Buat, who has taken up this matter where the Abbé Bossut left it, and has prosecuted his experiments with great assiduity; and we must now add with singular success. By a very judicious consideration of the subject, he hit on a particular view of it, which saved him the trouble of a minute consideration of the small internal motions, and enabled him to proceed from a very general and evident proposition, which may be received as the key to a complete system of practical hydraulics. We shall follow this ingenious author in what we have farther to say on the subject; and we doubt not but that our readers will think we do a service to the public by making these discussions of the chevalier de Buat more generally known in this country. It must not however be expected that we shall give more than a synoptical view of them, connected by such familiar reasoning as shall be either comprehended or confided in by persons not deeply versed in mathematical science.

SECT. I.—*Theory of Rivers.*

IT is certain that the motion of open streams must, in some respects, resemble that of bodies sliding down inclined planes perfectly polished; and that they would accelerate continually, were they not obstructed: but they are obstructed, and frequently move uniformly. This can only arise from an equilibrium between the forces which promote their descent and those which oppose it. M. Buat, therefore, assumes the leading proposition, that

When water flows uniformly on any channel or bed, the accelerating force which obliges it to move is equal to the sum of all the resistances which it meets with, whether *arising from its own viscidity, or from the friction of its bed.*

This law is as old as the formation of rivers, and should be the key of hydraulic science. Its evidence is clear; and it is, at any rate, the basis of all uniform motion. And since it is so, there must be some considerable analogy between the motion in pipes and in open channels. Both owe their origin to an inequality of pressure; both would accelerate continually, if nothing hindered; and both are reduced to uniformity by the viscidity of the fluid and the friction of the channel.

It will therefore be convenient to examine the phenomena of water moving in pipes by the action of its weight only along the sloping channel. But previous to this, we must take some notice of the obstruction to the entry of water into a channel of any kind, arising from the deflection of the many different filaments which press into the channel from the reservoir from every side. Then we shall be able to separate this diminution of motion from the sum total that is observed, and ascertain what part remains as produced by the subsequent obstructions.

We then shall consider the principle of uniform motion, the equilibrium between the power and the resistance. The power is the relative height of the column of fluid which tends to move along the inclined plane of its bed; the resistance is the friction of the bed, the viscidity of the fluid, and its adhesion to the sides. Here are necessarily combined a number of circumstances which must be gradually detached that we may see the effect of each, viz. the extent of the bed, its perimeter, and its slope. By examining the effects produced by variations of each of these separately, we discover what share each has in the general effect; and having thus analysed the complicated phenomenon, we shall be able to combine those its elements, and frame a formula which shall comprehend every circumstance, from the greatest velocity to the extinction of all motion, and from the extent of a river to the narrow dimensions of a quill. We shall compare this formula with a series of experiments in all this variety of circumstances, partly made by M. Buat, and partly collected from other authors; and we shall leave the reader to judge of the agreement.

Confident that this agreement will be found most satisfactory, we shall then proceed to consider very cursorily the chief varieties which nature or art may introduce into these beds, the different velocities of the same stream, the intensity of the resistance produced by the inertia of the materials of the channel, and the force of the current by which it continually acts on this channel, tending to change either its dimensions or its form. We shall endeavour to trace the origin of these great rivers which spread like the branches of a vigorous tree, and occupy the surface even of a vast continent. We shall follow them in their course, unfold all their windings, study their train, and regimen, and point out the law of its stability; and we shall investigate the causes of their deviations and wanderings.

The study of these natural laws pleases the mind: but it answers a still greater purpose; it enables us to assist nature, and to hasten her operations, which our wants and our impatience often find too slow. It enables us to command the elements, and to force them to administer to our wants and our pleasures.

We shall therefore, in the next place, apply the knowledge which we may acquire to the solution of the most important hydraulic questions which occur in the practice of the civil engineer.

We shall consider the effects produced by a permanent addition to any river or stream by the union of another, and the opposite effect produced by any draught or offset, showing the elevation or depression produced up the stream, and the change made in the depth and velocity below the addition or offset.

We shall pay a similar attention to the temporary swells produced by freshes.

We shall ascertain the effects of straightening the course of a stream, which, by increasing its slope, must increase its velocity, and therefore sink the waters above the place where the curvature was removed, and diminish the tendency to overflow, while the same immediate consequence must expose the places farther down to the risk of floods from which they would otherwise have been free.

The effects of dams or weirs, and of bars, must then be considered; the gorge or swell which they produce up the stream must be determined for every distance from the weir or bar. This will furnish us with rules for rendering navigable or floatable such waters as have too little depth or too great slope. And it will appear that immense advantages may be thus derived, with a moderate expense, even from trifling brooks, if we will relinquish all prejudices, and not imagine that such conveyance is impossible, because it cannot be carried on by such boats and small craft as we have been accustomed to look at.

The effects of canals of derivation, the rules or maxims of draining, and the general maxims of embankment, come in the next place; and our discussions will conclude with remarks on the most proper forms for the entry to canals, locks, docks, harbours, and mouths of rivers, the best shape for the starlings of bridges and of boats for inland navigations, and such like subordinate but interesting particulars, which will be suggested by the general thread of discussion.

It is considered as physically demonstrated, that water issuing from a small orifice in the bottom or side of a very large vessel, almost instantly acquires and maintains the velocity which a heavy body would acquire by falling to the orifice from the horizontal surface of the stagnant water. This we shall call its NATURAL VELOCITY. Therefore if we multiply the area of the orifice by this velocity, the product will be the bulk or quantity of the water which is discharged. This we may call the NATURAL EXPENSE of water, or the NATURAL DISCHARGE.

Let O represent the area or section of the orifice expressed in some known measure, and h its depth under the surface. Let g express the velocity acquired by a heavy body during a second by falling. Let V be the medium velocity of the water's motion, Q the quantity of water discharged during a second, and N the natural expense.

We know that V is equal to $\sqrt{2g} \times \sqrt{h}$. Therefore $N = O. \sqrt{2g}. \sqrt{h}$.

If these dimensions be all taken in English feet, we have have $\sqrt{2g}$ very nearly equal to 8; and therefore $V = 8\sqrt{h}$, and $N = O.\ 8\sqrt{h}$.

But in our present business it is much more convenient to measure every thing by inches. Therefore since a body acquires the velocity of 32 feet 2 inches in a second, we have $2g = 64$ feet 4 inches, or 772 inches, and $\sqrt{2g} = 27{,}78$ inches, nearly $27\frac{3}{4}$ inches.

Therefore $V = \sqrt{772}\sqrt{h}, = 27{,}78\sqrt{h}$, and $N = O. \sqrt{772}\sqrt{h}, = O.27{,}78\sqrt{h}$.

But it is also well known, that if we were to calculate the expense or discharge for every orifice by this simple rule, we should in every instance find it much greater than nature really gives us.

When water issues through a hole in a thin plate, the lateral columns, pressing into the hole from all sides, cause the issuing filaments to converge to the axis of the jet, and contract its dimensions at a little distance from the hole. And it is in this place of greatest contraction that the water acquires that velocity which we observe in our experiments, and which we assume as equal to that acquired by falling from the surface. Therefore, that our computed discharge may best agree with observation, it must be calculated on the supposition that the orifice is diminished to the size of this smallest section. But the contraction is subject to variations, and the dimensions of this smallest section are at all times difficult to ascertain with precision. It is therefore much more convenient to compute from the real dimensions of the orifice, and to correct this computed discharge, by means of an actual comparison of the computed and effective discharges in a series of experiments made in situations resembling those cases which most frequently occur in practice. This correction or its cause, or the mechanism of those internal motions, is generally called CONTRACTION by the writers on hydraulics; and it is not confined to a hole in a thin plate: it happens in some degree in all cases where fluids are made to pass through narrow places. It happens in the entry into all pipes, canals, and sluices; nay even in the passage of water over the edge of a board, such as is usually set up on the head of a dam or weir, and even when this is immersed in water on both sides, as in a bar or keep, frequently employed for raising the waters of the level streams in Flanders, in order to render them navigable. We mentioned an observation* of M. Buat to this effect, when he saw a gooseberry rise up from the bottom of the canal along the face of the bar, and then rapidly fly over its top. We have attempted to represent this motion of the filaments in these different situations.

Fig. 5. A shows the motion through a thin plate.

B shows the motion when a tube of about two diameters long is added, and when the water flows with a full mouth. This does not always happen in so short a pipe (and never in one that is shorter), but the water frequently detaches itself from the sides of the pipe, and flows with a contracted jet.

C shows the motion when the pipe projects into the inside of the vessel. In this case it is difficult to make it flow full.

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* See Resistance of Fluids.

D represents a mouth-piece fitted to the hole, and formed agreeably to that shape which a jet would assume of itself. In this case all contraction is avoided, because the mouth of this pipe may be considered as the real orifice, and nothing now diminishes the discharge but a trifling friction of the sides.

E shows the motion of water over a dam or weir, where the fall is free or unobstructed; the surface of the lower stream being lower than the edge or sole of the waste-board.

F is a similar representation of the motion of water over what we would call a *bar* or *keep*.

It was one great aim of the experiments of Michelotti and Bossut to determine the effects of contraction in these cases. Michellotti, after carefully observing the form and dimensions of the natural jet, made various mouth-pieces resembling it, till he obtained one which produced the smallest diminution of the computed discharge, or till the discharge computed for the area of its smaller end approached the nearest to the effective discharge. And he at last obtained one which gave a discharge of one 983, when the natural discharge would have been 1000. This piece was formed by the revolution of a trochoid round the axis of the jet, and the dimensions were as follow:

Diameter of the outer orifice = 36
———— inner orifice = 46
Length of the axis = 96

The results of the experiments of the Abbé Bossut and of Michelotti scarcely differ, and they are expressed in the following table:

N or the natural expense	10,000	$= 0.27,78\sqrt{h}$
Q for the thin plate, Fig. 5. A. almost at the surface	6526	$0.18,13\sqrt{h}$
Q for ditto at the depth of 8 feet	6195	$0.17,21\sqrt{h}$
Q for ditto at the depth of 16 feet	6173	$0.17,15\sqrt{h}$

Q for a tube two diameters long, Fig. 5. B.	·6125	0.[illegible] √h
Q for ditto projecting inwards and flowing full	·6814	0.18,[illegible] √h
Q for ditto with a contracted jet, Fig. 5. C.	·5137	0.14,27 √h
Q for the mouth-piece, Fig. 5. D.	·9831	0.[illegible],31 √h
Q for a weir, Fig. 5. E.	·9536	0.26,[illegible]
Q for a bar, Fig. 5. F.	·9730	0.27,03 [illegible]

The numbers in the last column of this little table are the cubical inches of water discharged in a second when the height *h* is one inch.

It must be observed that the discharges assigned here for the weir and bar relate only to the contractions occasioned by the passage over the edge of the board. The weir may also suffer a diminution by the contractions at its two ends, if it should be narrower than the stream, as is generally the case, because the two ends are commonly of square masonry or wood-work. The contraction there is nearly the same with that at the edge of a thin plate. But this could not be introduced into this table, because its effect on the expense is the same in quantity whatever is the length of the waste-board of the weir.

In like manner, the diminution of discharge through a sluice could not be expressed here. When a sluice is drawn up, but its lower edge still remains under water, the discharge is contracted both above and at the sides, and the diminution of discharge by each is in proportion to its extent. It is not easy to reduce either of these contractions to computation, but they may be very easily observed. We frequently can observe the water, at coming out of a sluice into a mill-course, quit the edge of the aperture, and show a part of the bottom quite dry. This is always the case when the velocity of efflux is considerable. When it is very moderate, this place is occupied by an eddy water almost stagnant. When the head of water is 8 or 10 inches, and runs off freely, the space left between it and the sides is about $1\frac{1}{2}$ inches. If the sides of the entry have a slope, this void space can never appear; but there is always this tendency to convergence, which diminishes the quantity of the discharge.

It will frequently abridge computation very much to consider the water discharged in these different situations as moving with a common velocity, which we conceive as produced not by a fall from the surface of the fluid (which is exact only when the expense is equal to the natural expense,) but by a fall h accommodated to the discharge: or it is convenient to know the height which would produce that very velocity which the water issues with in these situations.

And also when the water is observed to be actually moving with a velocity V, and we know whether it is coming through a thin plate, through a tube, over a dam, &c. it is necessary to know the pressure or HEAD OF WATER h which has actually produced this velocity. It is convenient therefore to have the following numbers in readiness:

h for the natural expense $= \frac{V^2}{772}$.

h for a thin plate - $= \frac{V^2}{296}$

h for a tube 2 diam. long $= \frac{V^2}{505}$

h for a dam or weir - $= \frac{V^2}{726}$

h for a bar - $= \frac{V^2}{746}$

It was necessary to premise these FACTS in hydraulics, that we may be able in every case to distinguish between the force expended in the entry of the water into the conduit or canal, and the force employed in overcoming the resistance along the canal, and in preserving or accelerating its motion in it.

The motion of running water is produced by two causes; 1. The action of gravity; and, 2. The mobility of the particles, which makes them assume a level in confined vessels, or determines them to move to that side where there is a defect of pressure. When the surface is level, every particle is at rest, being equally pressed in all directions; but if the surface is not level, not only does a particle on the very surface tend by its own weight towards the lower side, as a body would slide along an inclined plane, but there is a force, external to itself, arising from a superiority of pressure on the upper end of the surface, which pushes this superficial particle towards the lower end; and this is not peculiar to the superficial particles, but affects every particle within the mass of water. In the vessel ACDE (Fig. 6.), containing water with an inclined surface AE, if we suppose all frozen but the extreme columns AKHB, FGLE, and a connecting portion HKCDLG, it is evident, from hydrostatical laws, that the water on this connecting part will be pushed in the direction CD; and if the frozen mass BHGF were moveable, it would also be pushed along. Giving it fluidity will make no change in this respect; and it is indifferent what is the situation and shape of the connecting column or columns. The propelling force (MNF being horizontal) is the weight of column AMNB. The same thing will obtain wherever we select the vertical columns. There will always be a force tending to push every particle of water in the direction of the declivity. The consequence will be, that the water will sink at one end and rise at the other, and its surface will rest in the horizontal position *a* O *e*, cutting the former in its middle O. This cannot be unless there be not only a motion of perpendicular descent and ascent of the vertical columns, but also a real motion of translation from K towards L. It perhaps exceeds our mathematical skill to tell what will be the motion of each particle. Newton did not attempt it in his investigation of the motion of waves, nor is it at all necessary here. We may, however, acquire a very distinct notion of its general effect. Let OPQ be a vertical plane passing through the middle point O. It is evident that every particle in PQ, such as P, is pressed in the direction QD, with a force equal to the weight of a single row of particles, whose length is the difference between the columns BH and FG. The force acting on the particle Q is, in like manner, the weight of a row of particles =AC—ED. Now if OQ, OA, OE, be divided in the same ratio, so that all the figures ACDE, BHGF, &c. may be similar, we see that the force arising solely from the declivity, and acting on each particle on the plane OQ, is proportional to its depth under the surface, and that the row of particles ACQDE, BHPGF, &c. which is to be moved by it, is in the same proportion. Hence it unquestionably follows, that the accelerating force on each particle of the row is the same in all. Therefore the whole plane OQ tends to advance forward together with the same velocity; and in the instant immediately succeeding, all these particles would be found again in a vertical plane indefinitely near to OQ; and if we sum up the forces, we shall find them the same as if OQ were the opening of a sluice, having the water on the side of D standing level with O, and the water on the other side standing at the height AC. This result is extremely different from that of the hasty theory of Guglielmini. He considers each particle in OQ as urged by an accelerating force proportional to its depth, it is true; but he makes it *equal* to the weight of the row OP, and never recollects that the greatest part of it is balanced by an opposite pressure, nor perceives that the force which is not balanced must be distributed among a row of particles which varies in the same proportion with itself. When these two circumstances are neglected, the result must be incompatible with observation. When the balanced forces are taken into the account of pressure, it is evident that the surface may be supposed horizontal, and that motion should obtain in this case as well as in the case of a sloping surface: and indeed this is Guglielmini's professed theory, and what he highly values himself on. He announces this discovery of a new principle, which he calls the energy of deep waters, as an important addition to hydraulics. It is owing to this, says he, that the great rivers are not stagnant at their mouths, where they have no perceptible declivity of surface, but, on the contrary, have greater energy and velocity than farther up, where they are shallower. This principle is the basis of his improved theory of rivers, and is insisted on at great length by all the subsequent writers. Buffon, in his theory of the earth, makes much use of it. We cannot but wonder that it has been allowed a place in the theory of rivers given in the great *Encyclopédie* of Paris, and in an article having the signature (O) of D'Alembert. We have been very anxious to show the falsity of this principle, because we consider it as a mere subterfuge of Guglielmini, by which he was able to patch up the mathematical theory which he had so hastily taken from Newton or Galileo; and we think that we have secured our readers from being misled by it, when we show that this energy must be equally operative when the surface is on a dead level. The absurdity of this is evident. We shall see by and by, that deep waters, when in actual motion, have an energy not to be found in shallow running waters, by which they are enabled to continue that motion: but this is not a moving principle; and it will be fully explained, as an immediate result of principles, not vaguely conceived and indistinctly expressed, like this of Guglielmini, but easily understood, and appreciable with the greatest precision. It is an energy common to all great bodies. Although they lose as much momentum in surmounting any obstacle as small ones, they lose but a small portion of their velocity. At present, employed only in considering the progressive motion of an open stream, whose surface is not level, it is quite enough that we see that such a motion must obtain, and that we see that there are propelling forces; and that those forces arise *solely* from the want of a level surface, or from the slope of the surface; and that, with respect to any one particle, the force acting on it is proportional to the difference of a level between each of the two columns (one on each side of the particle) which produce it. Were the surface level, there would be no motion; if it is not level, there will be motion; and this motion will proportional to the want of level or the declivity of the surface: it is of no consequence whether the bottom be level or not, or what is its shape.

Hence we draw a fundamental principle, that *the motion of rivers depends entirely on the slope of the surface.*

The SLOPE or declivity of any inclined plane is not properly expressed by the difference of height alone of its extremities; we must also consider its length: and the measure of the slope must be such that it may be the same while the declivity is the same. It must therefore be the same over the whole of any one inclined plane. We shall answer these conditions exactly, if we take for the measure of a slope the fraction which expresses the elevation of one extremity above the other divided by the length of the plane. Thus $\frac{AM}{AF}$ will express the declivity of the plane AF.

If the water met with no resistance from the bed in which it runs, if it had no adhesion to its sides and bottom, and if its fluidity were perfect, its gravity would accelerate its course continually, and the earth and its inhabitants would be deprived of all the advantages which they derive from its numberless streams. They would run off so quickly, that our fields, dried up as soon as watered, would be barren and useless. No soil could resist the impetuosity of the torrents; and their accelerating force would render them a destroying scourge, were it not that, by kind Providence, the resistance of the bed, and the viscidity of the fluid, become a check which reins them in and sets bounds to their rapidity. In this manner the friction on the sides, which, by the viscidity of the water, is communicated to the whole mass, and the very adhesion of the particles to each other, and to the sides of the channel, are the causes which make the resistances bear a relation to the velocity; so that the resistances augmenting with the velocities, come at last to balance the accelerating force. Then the velocity now acquired is preserved, and the motion becomes uniform, without being able to acquire new increase, unless some change succeeds either in the slope or in the capacity of the channel. Hence arises the second maxim in the motion of rivers, that *when a stream moves uniformly, the resistance is equal to the accelerating force.*

As in the efflux of water through orifices, we pass over the very beginnings of the accelerated motion, which is a matter of speculative curiosity, and consider the motion in a state of permanency, depending on the head of water, the area of the orifice, the velocity, and the expense; so, in the theory of the uniform motion of rivers, we consider the slope, the transverse section or area of the stream, the uniform velocity, and the expense. It will be convenient to affix precise meanings to the terms which we shall employ.

The SECTION of a stream is the area of a plane perpendicular to the direction of the general motion.

The resistances arise ultimately from the action of the water on the internal surface of the channel, and must be proportional (*cæteris paribus*) to the extent of the action. Therefore if we unfold the whole edge of this section, which is rubbed as it were by the passing water, we shall have a measure of the extent of this action. In a pipe, circular or prismatical, the whole circumference is acted on; but in a river or canal ACDE (Fig. 6.) the horizontal line AOE, which makes the upper boundary of the section ACDE, is free from all action. The action is confined to the three lines AC, CD, DE. We shall call this line ACDE the BORDER of the section.

The MEAN VELOCITY is that with which the whole section, moving equally, would generate a solid equal to the expense of the stream. This velocity is to be found perhaps but in one filament of the stream, and we do not know in which filament it is to be found.

Since we are attempting to establish an empirical theory of the motion of rivers, founded entirely on experiment and palpable deductions from them; and since it is extremely difficult to make experiments on open streams which shall have a precision sufficient for such an important purpose—it would be a most desirable thing to demonstrate an exact analogy between the mutual balancing of the acceleration and resistance in pipes and in rivers; for in those we can not only make experiments with all the desired accuracy, and admitting precise measures, but we can make them in a number of cases that are almost impracticable in rivers. We can increase the slope of a pipe from nothing to the vertical position, and we can employ every desired degree of pressure, so as to ascertain its effect on the velocity in degrees which open streams will not admit. The Chevalier de Buat has most happily succeeded in this demonstration; and it is here that his good fortune and his penetration have done so much service to practical science.

Let AB (Fig. 7.) be a horizontal tube, through which the water is impelled by the pressure or HEAD DA. This head is the moving power; and it may be conceived as consisting of two parts, performing two distinct offices. One of them is employed in impressing on the water that velocity with which it *actually moves* in the tube. Were there no obstructions to this motion, no greater head would be wanted; but there are obstructions arising from friction, adhesion, and viscidity. This requires force. Let this be the office of the rest of the head of water in the reservoir. There is but one allotment, appropriation, or repartition, of the whole head which will answer. Suppose E to be the point of partition, so that DE is the head necessary for impressing the actual velocity on the water (a head or pressure which has a relation to the form or circumstance of the entry, and the contraction which takes place there). The rest EA is wholly employed in overcoming the simultaneous resistances which take place along the whole tube AB, and is in equilibrio with this resistance. Therefore if we apply at E a tube EC of the same length and diameter with AB, and having the same degree of polish or roughness; and if this tube be inclined in such a manner that the axis of its extremity may coincide with the axis of AB in the point C—we affirm that the velocity will be the same in both pipes, and that they will have the same expense; for the moving force in the sloping pipe EC is composed of the whole weight of the column DE and the relative weight of the column EC; but this relative weight, by which alone it descends along the inclined pipe EC, is precisely equal to the weight of a vertical column EA of the same diameter. Every thing therefore is equal in the two pipes, viz. the lengths, the diameters, the moving forces, and the resistances; therefore the velocities and discharges will also be equal.

This is not only the case on the whole, but also in every part of it. The relative weight of any part of it EK is precisely in equilibrio with the resistances along that part of the pipe; for it has the same proportion to the whole relative weight that the resistance has to the whole resistance. Therefore (*and this is the most important circumstance, and the basis of the whole theory*) the pipe EC may be cut shorter, or may be lengthened to infinity, without making any change in the velocity or expense, so long as the propelling head DE remains the same.

Leaving the whole head DA as it is, if we lengthen the horizontal pipe AB to G, it is evident that we increase the resistance without any addition of force to overcome it. The velocity must therefore be diminished; and it will now be a velocity which is produced by a smaller head than DE: therefore if we were to put in a pipe of equal length at E, terminating in the horizontal line AG, the water will not run equally in both pipes. In order that it may, we must discover the diminished velocity with which the water now actually runs along AG, and we must make a head DI capable of impressing this velocity at the entry of the pipe, and then insert at I a pipe IH of the same length with AG. The expense and velocity of both pipes will now be the same.*

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* We recommend it to the reader to make this distribution or allotment of the different portions of the pressure very familiar to his mind. It is of the most extensive influence in every question of hydraulics, and will on every occasion give him distinct conceptions of the internal procedure. Obvious as the thought seems to be, it has escaped the attention of all the writers on the subject. Lecchi, in his *Hydraulics*, published in 1766, ascribes something like it to Daniel Bernoulli; but Bernoulli, in the passage quoted, only speaks of the partition of pressure in the instant of opening an orifice. Part of it, says he, is employed in accelerating the quiescent water, and producing the velocity of efflux, and the remainder produces the pressure (now diminished) on the sides of the vessel. Bernoulli, Bossut, and all the good writers, make this distribution in express terms in their explanation of the motion of water through successive orifices; and it is surprising that no one before the Chevalier de Buat saw that the resistance arising from friction required a similar partition of the pressure; but though we should call this good fortune, we must ascribe to his great sagacity and justness of conception the beautiful use that he has made of it.

What has now been said of a horizontal pipe AB would have been equally true of any inclined pipe AB, A′B (Fig. 8.) Drawing the horizontal line CB, we see that DC is the whole head or propelling pressure for either pipe AB or A′B; and if DE is the head necessary for the actual velocity, EC is the head necessary for balancing the resistances; and the pipe EF of the same length with AB, and terminating in the same horizontal line, will have the same velocity; and its inclination being thus determined, it will have the same velocity and expense whatever be its length.

Thus we see that the motion in any pipe, horizontal or sloping, may be referred to or substituted for the motion in another inclined pipe, whose head of water, above the place of entry, is that productive of the actual velocity of the water in the pipe. Now, in this case, the accelerating force is equal to the resistance: we may therefore consider this last pipe as a river, of which the bed and the slope are uniform or constant, and the current in a state of permanency; and we now may clearly draw this important conclusion, that pipes and open streams, when in a state of permanency, perfectly resemble each other in the circumstances which are the immediate causes of this permanency. The equilibrium between the accelerating force obtains not only in general, but takes place through the whole length of the pipe or stream, and is predicable of every individual transverse section of either. To make this more palpably evident if possible, let us consider a sloping cylindrical pipe, the current of which is in a state of permanency. We can conceive it as consisting of two half cylinders, an upper and a lower. These are running together at an equal pace; and the filaments of each immediately contiguous to the separating plane and to each other, are not rubbing on each other, nor affecting each other's motions in the smallest degree. It is true that the upper half is pressing on the lower, but in a direction perpendicular to the motion, and therefore not affecting the velocity; and we shall see presently, that although the lower side of the pipe bears somewhat more pressure than the other, the resistances are not changed. (Indeed this odds of pressure is accompanied with a difference of motion, which need not be considered at present; and we may suppose the pipe so small or so far below the surface, that this shall be insensible). Now let suppose, that in an instant the upper half cylinder is annihilated: we then have an open stream; and every circumstance of accelerating force and of resistance remains precisely as it was. The motion must therefore continue as it did; and in this state the only accelerating force is the slope of the surface. The demonstration therefore is complete.

From these observations and reasonings, we draw a general and important conclusion, " That the same pipe will be susceptible of different velocities, which it will preserve uniform to any distance, according as it has different inclinations; and each inclination of a pipe of given diameter has a certain velocity peculiar to itself which will be maintained uniform to any distance whatever; and this velocity increases continually, according to some law, to be discovered by theory or experiment, as the position of the pipe changes, from being horizontal till it becomes vertical; in which position it has the greatest uniform velocity possible relative to its inclination, or depending on inclination alone.

Let this velocity be called the TRAIN, or the RATE of each pipe.

It is evident that this principle is of the utmost consequence in the theory of hydraulics; for by experiment we can find the train of any pipe. It is in train when an increase of length makes no change in the velocity. If lengthening the pipe increases the velocity, the slope of the pipe is too great, and *vice versa*. And having discovered the train of a pipe, and observed its velocity, and computed the head productive of this velocity with the contraction at the entry, the remainder of the head, that is, the slope (for this is equivalent to EA), is the measure of the resistance. Thus we obtain the measure of the resistance to the motion with a given velocity in a pipe of given diameter. If we change only the velocity, we get the measure of the new resistance relative to the velocity; and thus discover the law of relation between the resistance and velocity. Then, changing only the diameter of the pipe, we get the measure of the resistance relative to the diameter. This is the aim of a prodigious number of experiments made and collected by Buat, and which we shall not repeat, but only give the results of the different parts of his investigation.

We may express the slope of a pipe by the symbol $\frac{1}{s}$, 1 being an inch, for instance, and s being the slant length of a pipe which is one inch more elevated at one end than at the other. Thus a river which has a declivity of an inch and a half in 120 fathoms, or 8640 inches, has its slope $= \frac{1\frac{1}{2}}{8640}$, or $\frac{1}{5760}$. But in order to obtain the hydraulic slope of a conduit pipe, the heights of the reservoir and place of discharge being given, we must subtract from the difference of elevation the height or head of water necessary for propelling the water into any pipe with the velocity V, which it is supposed actually to have. This is $\frac{V^2}{505}$. The remainder d is to be considered as the height of the declivity, which is to be distributed equally over the whole length l of the pipe, and the slope is then $\frac{d}{l}$, $= \frac{1}{s}$.

There is another important view to be taken of the slope, which the reader should make very familiar to his thoughts.

It expresses the proportion between the weight of the whole column which is in motion and the weight which is employed in overcoming the resistance; and the resistance to the motion of any column of water is equal to the weight of that column multiplied by the fraction $\frac{1}{s}$, which expresses its slope.

We come now to consider more particularly the resistances which in this manner bring the motions to a state of uniformity. If we consider the resistances which arise from a cause analogous to friction, we see that they must depend entirely on the inertia of the water. What we call the resistance is the diminution of a motion which *would* have obtained but for these resistances; and the best way we have of measuring them is by the force which we must employ in order to keep up or restore this motion. We estimate this motion by a progressive velocity, which we measure by the expense of water in a given time. We judge the velocity to diminish, when the quantity discharged diminishes; yet it may be otherwise, and probably is otherwise. The absolute velocity of many, if not all of the particles, may even be increased; but many of the motions, being transverse to the general direction, the quantity of motion in this direction may be less, while the sum of the absolute motions of all the particles may be greater. When we increase the general velocity, it is not unreasonable to suppose that the impulses on all the inequalities are increased in this proportion; and the number of particles thus impelling and deflected at the same time will increase in the same proportion. The whole quantity therefore of these useless and lost motions will increase in the duplicate ratio of the velocities, and the force necessary for keeping up the motion will do so also; that is, the resistances should increase as the squares of the velocities.

Or if we consider the resistances as arising merely from the curvature of the imperceptible internal motions occasioned by the inequalities of the sides of the pipe, and as measured by the forces necessary for producing these curvilineal motions; then, because the curves will be the same whatever are the velocities, the deflecting forces will be as the squares of the velocities; but these deflecting forces are pressures, propagated from the parts urged or pressed by the external force, and are proportional to these external pressures by the principles of hydrostatics. Therefore the pressures or forces necessary for keeping up the velocities are as the squares of these velocities; and they are our only measures of the resistances which must be considered as following the same ratio. Whatever view therefore we take of the nature of these resistances, we are led to consider them as proportional to the squares of the velocities.

We may therefore express the resistances by the symbol $\frac{V^2}{m}$, m being some number to be discovered by experiment. Thus, in a particular pipe, the diminution of the motion or the resistance may be 1000th part of the square of the velocity, and $R = \frac{V^2}{1000}$.

Now if g be the accelerating power of gravity on any particle $\frac{g}{s}$ will be its accelerating power, by which it would urge it down the pipe whose slope is $\frac{1}{s}$. Therefore, by the principle of uniform motion, the equality of the accelerating force, and the resistance, we shall have $\frac{V^2}{m} = \frac{g}{s}$; and $V \sqrt{s} = \sqrt{mg}$; that is, the product of the velocity, and the reciprocal of the square root of the slope, or the quotient of the velocity divided by the slope, is a constant quantity $\sqrt{mg}$ for any given pipe; and the priinary formula for all the uniform velocities of one pipe is $V = \frac{\sqrt{mg}}{\sqrt{s}}$.

M. Buat therefore examined this by experiment, but found, that even with respect to a pipe or channel, which was uniform throughout, this was not true. We could give at once the final formula which he found to express the velocity in every case whatever; but this would be too empirical. The chief steps of his very sagacious investigation are instructive. We shall therefore mention them briefly, at least as far as they tend to give us any collateral information; and let it always be noted, that the instruction which they convey is not abstract speculation, but experimental truths, which must ever remain as an addition to our stock of knowledge, although M. Buat's deductions from them should prove false.

He found, in the first place, that in the same channel the product of V and $\sqrt{s}$ increased as $\sqrt{s}$ increased; that is, the velocities increased faster than the square roots of the slope, or the resistances did not increase as fast as the squares of the velocities. We beg leave to refer our readers to what we said on the resistance of pipes to the motion of fluids through them in the article Pneumatics, when speaking of bellows. They will there see very valid reasons (we apprehend) for thinking that the resistances must increase more slowly than the squares of the velocities.

It being found, then, that $V\sqrt{s}$ is not equal to a constant quantity $\sqrt{mg}$, it becomes necessary to investigate some quantity depending on $\sqrt{s}$, or, as it is called, some function of $\sqrt{s}$, which shall render $\sqrt{mg}$ a constant quantity. Let X be this function of $\sqrt{s}$, so that we shall always have VX equal to the constant quantity $\sqrt{mg}$, or

$\frac{\sqrt{mg}}{X}$, equal to the actual velocity V of a pipe or channel which is in train.

M. Buat, after many trials and reflections, the chief of which will be mentioned by and by, found a value of X which corresponded with a vast variety of slopes and velocities, from motions almost imperceptible, in a bed nearly horizontal, to the greatest velocities which could be produced by gravity alone in a vertical pipe; and when he compared them together, he found a very discernible relation between the resistances and the magnitude of the section; that is, that in two channels which had the same slope, and the same propelling force, the velocity was greatest in the channel which had the greatest section relative to its border. This may reasonably be expected. The resistances arise from the mutual action of the water and this border. The water immediately contiguous to it is retarded, and this retards the next, and so on. It is to be expected, therefore, that if the border, and the velocity, and the slope, be the same, the diminution of this velocity will be so much the less as it is to be shared among a greater number of particles; that is, as the area of the section is greater in proportion to the extent of its border. The diminution of the general or medium velocity must be less in a cylindrical pipe than in a square one of the same area, because the border of its section is less.

It appears evident, that the resistance of each particle is in the direct proportion of the whole resistance, and the inverse proportion of the number of particles which receive equal shares of it. It is therefore directly as the border, and inversely as the section. Therefore in the expression $\frac{V^2}{m}$ which we have given for the resistance, the quantity m cannot be constant, except in the same channel; and in different channels it must vary along with the relation of the section to its border, because the resistances diminish in proportion as this relation increases.

Without attempting to discover this relation by theoretical examination of the particular motions of the various filaments, M. Buat endeavoured to discover it by a comparison of experiments. But this required some manner of stating this proportion between the augmentation of the section and the augmentation of its border.

His statement is this: He reduces every section to a rectangular parallelogram of the same area, and having its base equal to the border unfolded into a straight line. The product of this base by the height of the rectangle will be equal to the area of the section. Therefore this height will be a representative of this variable ratio of the section to its border (We do not mean that there is any ratio between a surface and a line: but the ratio of section to section is different from that of border to border; and it is the ratio of these ratios which is thus expressed by the height of this rectangle). If S be the section, and B the border, $\frac{S}{B}$ is evidently a line equal to the height of this rectangle. Every section being in this manner reduced to a rectangle, the perpendicular height of it may be called the HYDRAULIC MEAN DEPTH of the section, and may be expressed by the symbol d. (Buat calls it the mean radius). If the channel be a cylindrical pipe, or an open half cylinder, it is evident that d is half the radius. If the section is a rectangle, whose width is w, and height h, the mean depth is $\frac{wh}{b+2h}$, &c. In general, if q represent the proportion of the breadth of a rectangular canal to its depth, that is, if q be made $= \frac{w}{h}$, we shall have $d = \frac{w}{q+2}$, or $d = \frac{qh}{q+2}$.

Now, since the resistances must augment as the proportion of the border to the section augments, as in the formulas $\frac{V^2}{m} = \frac{g}{s}$ and $V \sqrt{s} = \sqrt{mg}$, must follow the proportions of d, and the quantity $\sqrt{mg}$ must be proportional to $\sqrt{d}$ for different channels, and $\frac{\sqrt{mg}}{\sqrt{d}}$ should be a constant quantity in every case.

Our author was aware, however, of a very specious objection to the close dependence of the resistance on the extent of the border; and that it might be said that a double border did not occasion a double resistance, unless the pressure on all the parts was the same. For it may be naturally (and it is generally) supposed, that the resistance will be greater when the pressure is greater. The friction or resistance analogous to friction may therefore be greater on an inch of the bottom than on an inch of the sides; but M. D'Alembert and many others have demonstrated, that the paths of the filaments will be the same, whatever be the pressures. This might serve to justify our ingenious author; but he was determined to rest every thing on experiment. He therefore made an experiment on the oscillation of water in syphons, which we have repeated in the following form, which is affected by the same circumstances, and is susceptible of much greater precision, and of more extensive and important application:

The two vessels ABCD, *a b c d* (Fig. 9.) were connected by the syphon EFG*gfe*, which turned round in the short tubes E and *e*, without allowing any water to escape; the axes of these tubes being in one straight line. The vessels about ten inches deep, and the branches FG, *fg* of the syphon were about five feet long. The vessels were set on two tables of equal height, and (the hole *e* being stopped) the vessel ABCD, and the whole syphon, were filled with water, and water was poured into the vessel *a b c d* till it stood at a certain height *e* M. The syphon was then turned into a horizontal position, and the plug drawn out of *e*, and the time carefully noted which the water employed in rising to the level HK*kh* in both vessels. The whole apparatus was now inclined, so that the water run back into ABCD. The syphon was now put in a vertical position, and the experiment was repeated.—No sensible or regular difference was observed in the time. Yet in this experiment the pressure on the part G*g* of the syphon was more than six times greater than before. As it was thought that the friction on this small part (only six inches) was too small a portion of the whole obstruction, various additional obstructions were put into this part of the syphon, and it was even lengthened to nine feet; but still no remarkable difference was observed. It was even thought that the times were less when the syphon was vertical.

Thus M. de Buat's opinion is completely justified; and he may be allowed to assert, that the resistance depends chiefly on the relation between the section and its border; and that $\frac{\sqrt{mg}}{\sqrt{d}}$ should be a constant quantity.

To ascertain this point was the object of the next series of experiments; to see whether this quantity was really constant, and, if not, to discover the law of its variation, and the physical circumstances which accompanied the variations, and may therefore be considered as their causes. A careful comparison of a very great number of experiments, made with the same slope, and with very different channels and velocities, showed that $\sqrt{mg}$ did not follow the proportion of $\sqrt{d}$, nor of any power of $\sqrt{d}$. This quantity $\sqrt{mg}$ increased by smaller degrees in proportion as $\sqrt{d}$ was greater. In very great beds $\sqrt{mg}$ was near-proportional to $\sqrt{d}$, but in smaller channels, the velocities diminished much more than $\sqrt{d}$ did. Casting about for some way of accommodation, M. Buat considered, that some approximation at least would be had by taking off from $\sqrt{d}$ some constant small quantity. This is evident: for such a diminution will have but a trifling effect when $\sqrt{d}$ is great, and its effect will increase rapidly when $\sqrt{d}$ is very small. He therefore tried various values for this subtraction, and compared the results with the former experiments; and he found, that if in every case $\sqrt{d}$ be diminished by one-tenth of an inch, the calculated discharges would agree very exactly with the experiment. Therefore, instead of $\sqrt{d}$, he makes use of $\sqrt{d} - 0{,}1$, and finds this quantity always proportional to $\sqrt{mg}$, or finds that $\frac{\sqrt{mg}}{\sqrt{d} - 0{,}1}$ is a constant quantity, or very nearly so. It varied from 297 to 287 in all sections from that of a very small pipe to that of a little canal. In the large sections of canals and rivers it diminished still more, but never was less than 256.

This result is very agreeable to the most distinct notions that we can form of the mutual actions of the water and its bed. We see, that when the motion of water is obstructed by a solid body, which deflects the passing filaments, the disturbance does not extend to any considerable distance on the two sides of the body. In like manner, the small disturbances, and imperceptible curvilineal motions which are occasioned by the infinitesimal inequalities of the channel, must extend to a very small distance indeed from the sides and bottom of the channel. We know, too, that the mutual adhesion or attraction of water for the solid bodies which are moistened by it, extends to a very small distance; which is probably the same, or nearly so, in all cases. M. Buat observes, that a surface of 23 square inches, applied to the surface of stagnant water, lifted 1601 grains; another of 5½ square inches lifted 365: this was at the rate of 65 grains per inch nearly, making a column of about one-sixth of an inch high. Now this effect is very much analogous to a real contraction of the capacity of the channel. The water may be conceived as nearly stagnant to this small distance from the border of the section. Or, to speak more accurately, the diminution of the progressive velocity occasioned by the friction and adhesion of the sides, decreases very rapidly as we recede from the sides, and ceases to be sensible at a very small distance.

The writer of this article verified this by a very simple and instructive experiment. He was making experiments on the production of vortices, in the manner suggested by Sir Isaac Newton, by whirling a very accurate and smoothly polished cylinder in water; and he found that the rapid motion of the surrounding water was confined to an exceeding small distance from the cylinder, and it was not till after many revolutions that it was sensible even at the distance of half an inch. We may, by the way, suggest this as the best form of experiments for examining the resistances of pipes. The motion excited by the whirling cylinder in the stagnant water is equal and opposite to the motion lost by water passing along a surface equal to that of the cylinder with the same velocity. Be this as it may, we are justified in considering, with M. Buat, the section of the stream as thus diminished by cutting off a narrow border all round the touching parts, and supposing that the motion and discharge is the same as if the root of the mean depth of the section were diminished by a small quantity, nearly constant. We see, too, that the effect of this must be insensible in great canals and rivers; so that, fortunately, its quantity is best ascertained by experiments made with small pipes. This is attended with another conveniency, in the opinion of M. Buat, namely, that the effect of viscidity is most sensible in great masses of water in slow motion, and is almost insensible in small pipes, so as not to disturb these experiments. We may therefore assume 297 as the general value of $\frac{\sqrt{mg}}{\sqrt{d}-0,1}$.

Since we have $\frac{\sqrt{mg}}{\sqrt{d}-0,1} = 297$, we have also $m = \frac{\overline{297}^2}{g} \times \overline{\sqrt{d}-0,1}^2, = \frac{88209}{362} (\sqrt{d} - 0,1)^2, = 243,7$ $(\sqrt{d}-0,1)^2$. This we may express by n $(\sqrt{d}-0,1)^2$. And thus, when we have expressed the effect of friction by $\frac{V^2}{m}$, the quantity m is variable, and its general value is n $\frac{V^2}{(\sqrt{d}-0,1)^2}$, in which n is an invariable abstract number equal to 243,7, given by the nature of the resistance which water sustains from its bed, and which indicates its intensity.

And, lastly, since $m = n (\sqrt{d}-0,1)^2$, we have $\sqrt{mg}$ $= \sqrt{ng} (\sqrt{d}-0,1)$, and the expression of the velocity V, which water acquires and maintains along any channel whatever, now becomes $V = \frac{\sqrt{ng}\,(\sqrt{d}-0,1)}{X}$, or $\frac{297\,(\sqrt{d}-0,1)}{X}$, in which X is also a variable quantity, depending on the slope of the surface or channel, and expressing the accelerating force which, in the case of water in train, is in equilibrio with the resistances expressed by the numerator of the fraction.

Having so happily succeeded in ascertaining the variations of resistance, let us accompany M. Buat in his investigation of the law of acceleration, expressed by the value of X.

Experience, in perfect agreement with any distinct opinions that we can form on this subject, had already showed him, that the resistances increased in a slower ratio than that of the squares of the velocities, or that the velocities increased slower than $\sqrt{s}$. Therefore, in the formula V $= \frac{\sqrt{ng}\,(\sqrt{d}-0,1)}{X}$, which, for one channel, we may express thus, $V = \frac{A}{X}$, we must admit that X is sensibly equal to $\sqrt{s}$ when the slope is very small or s very great. But, that we may accurately express the velocity in proportion as the slope augments, we must have X greater than $\sqrt{s}$; and moreover, $\frac{\sqrt{s}}{X}$ must increase as $\sqrt{s}$ diminishes. These conditions are necessary, that our values of V, deduced from the formula $V = \frac{A}{X}$, may agree with the experiment.

In order to comprehend every degree of slope, we must particularly attend to the motion through pipes, because open canals will not furnish us with instances of exact TRAINS with great slopes and velocities. We can make pipes vertical. In this case $\frac{1}{s}$ is $\frac{1}{1}$, and the velocity is the greatest possible for a train by the action of gravity: but we can give greater velocities than this by increasing the head of water beyond what produces the velocity of the train.

Let AB (Fig. 10.) be a vertical tube, and let CA be the head competent to the velocity in the tube, which we suppose to be in train. The slope is 1, and the full weight of the column in motion is the precise measure of the resistance. The value of $\frac{1}{s}$, considered as a slope, is now a maximum; but, considered as expressing the proportion of the weight of the column in motion to the weight which is in equilibrio with the resistance, it may not be a maximum; it may surpass unity, and s may be less than 1. For if the vessel be filled to E, the head of water is increased, and will produce a greater velocity, and this will produce a greater resistance. The velocity being now greater, the head EF which imparts it must be greater than CA. But it will not be equal to EA, because the uniform velocities *are found* to increase faster than the square roots of the pressures. This is the general fact. Therefore F is above A, and the weight of the column FB, now employed to overcome the resistance, is greater than the weight of the column AB in motion. In such cases, therefore, $\frac{1}{s}$, greater than unity, is a sort of fictitious slope, and only represents the proportion of the resistance to the weight of the moving column. This proportion may surpass unity.

But it cannot be infinite: for supposing the head of water infinite; if this produçe a finite velocity, and we deduct from the whole height the height corresponding to this finite velocity, there will remain an infinite head, the measure of an infinite resistance produced by a finite velocity. This does not accord with the observed law of the velocities, where the resistances actually do not increase as fast as the squares of the velocities. Therefore an infinite head would have produced an infinite velocity, in opposition to the resistances: taking off the head of the tube, competent to this velocity, at the entry of the tube, which head would also be infinite, the remainder would in all probability be finite, balancing a finite resistance.

Therefore, the value of s may remain finite, although the velocity be infinite; and this is agreeable to all our clearest notions of the resistances.

Adopting this principle, we must find the value of X which will answer all these conditions. 2. It must be sensibly proportional to $\sqrt{s}$, while s is great. It must always be less than $\sqrt{s}$. 3. It must deviate from the proportion of $\sqrt{s}$, so much the more as $\sqrt{s}$ is smaller. 4. It must not vanish when the velocity is infinite. 5. It must agree with a range of experiments with every variety of channel and of slope.

We shall understand the nature of this quantity X better by representing by lines the quantities concerned in forming it.

If the velocities were exactly as the square roots of the slopes, the equilateral hyperbola NKS (Fig. 11.) between its assymptotes MA, AB, would represent the equation $V = \frac{A}{\sqrt{s}}$. The values of $\sqrt{s}$ would be represented by the abscissæ, and the velocities by the ordinates and $V\sqrt{s} = A$ would be the power of the hyperbola. But since these velocities are not sensibly equal to $\frac{A}{\sqrt{s}}$ except when $\sqrt{s}$ is very great, and deviate the more from this quantity as $\sqrt{s}$ is smaller; we may represent the velocities by the ordinates of another curve PGT, which approaches very near to the hyperbola, at a great distance from A along AB; but separates from it when the abscissa are smaller: so that if AQ represents that value of $\sqrt{s}$ (which we have seen may become less than unity), which corresponds to an infinite velocity, the line QO may be the assymptote of the new curve. Its ordinates are equal to $\frac{A}{X}$ while those of the hyperbola are equal to $\frac{A}{\sqrt{s}}$. Therefore the ratio of these ordinates or $\frac{\sqrt{s}}{X}$ should be such that it shall be so much nearer to unity as $\sqrt{s}$ is greater, and shall surpass it so much the more as $\sqrt{s}$ is smaller.

To express X therefore as some function of $\sqrt{s}$ so as to answer these conditions, we see in general that X must be less than $\sqrt{s}$. And it must not be equal to any power of $\sqrt{s}$ whose index is less than unity, because then $\frac{\sqrt{s}}{X}$ would differ so much the more from unity as $\sqrt{s}$ is greater. Nor must it be any multiple of $\sqrt{s}$ such as $q\sqrt{s}$, for the same reason. If we make $X = \sqrt{s} - K$, K being a constant quantity, we may answer the first condition pretty well. But K must be very small, that X may not become equal to nothing, except in some exceedingly small value of $\surd s$. Now the experiments will not admit of this, because the ratio $\frac{\surd s}{\surd s - K}$ does not increase sufficiently to correspond with the velocities which we observe in certain slopes, unless we make K greater than unity, which again is inconsistent with other experiments. We learn from such canvassing that it will not do to make K a constant quantity. If we should make it any fractionary power of $\surd s$, it would make $X = 0$, that is, nothing, when $\surd s = 1$, which is also contrary to experience. It would seem therefore, that nothing will answer for K but some power of $\surd s$ which has a variable index. The logarithm of $\surd s$ has this property. We may therefore try to make $X = \surd s - \text{log.}\ \surd s$. Accordingly if we try the equation $V = \frac{A}{\surd s - \text{hyp. log.}\ \surd s}$, we shall find a very great agreement with the experiments till the declivity becomes considerable, or about $\frac{1}{50}$, which is much greater than any river. But it will not agree with the velocities observed in some mill-courses, and in pipes of a still greater declivity, and gives a velocity that is too small; and in vertical pipes the velocity is not above one half of the true one. We shall get rid of most of these incongruities if we make K consist of the hyperbolic logarithm of $\surd s$ augmented by a small constant quantity, and by trying various values for this constant quantity, and comparing the results with experiments, we may hit on one sufficiently exact for all practical purposes.

M. de Buat, after repeated trials, found that he would have a very great conformity with experiment by making $K = \text{log.}\ \sqrt{s + 1,6}$, and that the velocities exhibited in his experiments would be very well represented by the formula $V = \frac{297 (\surd d - 0,1)}{\sqrt{s} - L \sqrt{s + 1,6}}$.

There is a circumstance which our author seems to have overlooked on this occasion, and which is undoubtedly of great effect in these motions, viz. the mutual adhesion of the particles of water. This causes the water which is descending (in a vertical pipe for example) to drag more water after it, and thus greatly increases its velocity. We have seen an experiment in which the water issued from the bottom of a reservoir through a long vertical pipe having a very gentle taper. It was 15 feet long, one inch diameter at the upper end, and two inches at the lower. The depth of the water in the reservoir was exactly one foot; in a minute there were discharged $2\frac{9}{10}$ cubic feet of water. It must therefore have issued through the hole in the bottom of the reservoir with the velocity of 8,85 feet per second. And yet we know that this head of water could not make it pass through the hole with a velocity greater than 6,56 feet per second. This increase must therefore have arisen from the cause we have mentioned, and is a proof of the great intensity of this force. We doubt not but that the discharge might have been much more increased by proper contrivances; and we know many instances in water-pipes where this effect is produced in a very great degree.

The following case is very distinct: Water is brought into the town of Dunbar, in the county of East Lothian, from a spring at the distance of about 3200 yards. It is conveyed along the first 1100 yards in a pipe of two inches diameter, and the declivity is 12 feet 9 inches; from thence the water flows in a pipe of 1¼ diameter, with a declivity of 44 feet 3 inches, making in all 57 feet. When the work was carried as far as the two-inch pipe reached, the discharge was found to be 27 Scotch pints, of 103¼ cubic inches each in a minute. When it was brought into the town, the discharge was 28. Here it is plain, that the descent along the second stretch of the pipe could derive no impulsion from the first. This was only able to supply 27 pints, and to *deliver* it into a pipe of equal bore. It was not equivalent to the forcing it into a smaller pipe, and almost doubling its velocity. It must, therefore, have been *dragged* into this smaller pipe by the weight of what was descending along it; and this water was exerting a force equivalent to a head of 16 inches, increasing the velocity from 14 to about 28.

'It must be observed, that if this formula be just, there can be no declivity so small that a current of water will not take place in it. And accordingly none has been observed in the surface of a stream when this did not happen. But it also should happen with respect to any declivity of bottom. Yet we know that water will hang on the sloping surface of a board without proceeding further. The cause of this seems to be the adhesion of the water combined with its viscidity. The viscidity of a fluid presents a certain force which must be overcome before any current can take place.'

'A series of important experiments were made by our author in order to ascertain the relation between the velocity at the surface of any stream and that at the bottom. These are curious and valuable on many accounts. One circumstance deserves our notice here, viz. that *the difference between the superficial and bottom velocities of any stream are proportional to the square roots of the superficial velocities.* From what has been already said on the gradual diminution of the velocities among the adjoining filaments, we must conclude that the same rule holds good with respect to the velocity of separation of two filaments immediately adjoining. Hence we learn that this velocity of separation is in all cases indefinitely small, and that we may, without danger of any sensible error, suppose it a constant quantity in all cases.

We think, with our ingenious author, that on a review of these circumstances, there is a constant or invariable portion of the accelerating force employed in overcoming this viscidity and producing this mutual separation of the adjoining filaments. We may express this part of the accelerating force by a part $\frac{1}{S}$ of that slope which constitutes the whole of it. If it were not employed in overcoming this resistance, it would produce a velocity which (on account of this resistance) is not produced, or is lost. This would be $\frac{A}{\sqrt{S}-L\sqrt{S}}$. This must therefore be taken from the velocity exhibited by our general formula. When thus corrected, it would become $V = (\sqrt{d}-0,1)\left(\frac{\sqrt{ng}}{\sqrt{s}-L\sqrt{s+1,6}}-\frac{\sqrt{ng}}{\sqrt{S}-L\sqrt{S}}\right)$. But as the term $\frac{\sqrt{ng}}{\sqrt{S}-L\sqrt{S}}$ is compounded only of constant quantities, we may express it by a single number. This has been collected from a scrupulous attention to the experiments (especially in canals and great bodies of water moving with very small velocities; in which case the effects of viscidity must become more remarkable), and it appears that it may be valued at $\sqrt{0,09 \text{ inch}}$, or 0,3 inch, very nearly.

From the whole of the foregoing considerations, drawn from nature, supported by such reasoning as our most distinct notions of the internal motions will admit, and authorised by a very extensive comparison with experiment, we are now in a condition to conclude a complete formula, expressive of the uniform motion of water, and involving every circumstance which appears to have any share in the operation.

Therefore let

V represent the mean velocity, in inches per second, of any current of water, running uniformly, or which is IN tram, in a pipe or open channel, whose section, figure, and slope, are constant, but its length indefinite.

d the hydraulic mean depth, that is, the quotient arising from dividing the section of the channel, in square inches, by its border, expressed in linear inches.

s, the slope of the pipe, or of the surface of the current. It is the denominator of the fraction expressing this slope, the numerator being always unity; and is had by dividing the expanded length of the pipe, or channel by the difference of height of its two extremities.

g the velocity (in inches per second) which a heavy body acquires by falling during one second.

n an abstract constant number, determined by experiment to be 243,7.

L the hyperbolic logarithm of the quantity to which it is prefixed, and is had by multiplying the common logarithm of that quantity by 2,3026.

We shall have in every instance

$$V = \frac{\sqrt{ng}\,(\sqrt{d}-0,1)}{\sqrt{s}-L.\sqrt{s+1,6}} - 0,3\,(\sqrt{d}-0,1)$$

This, in numbers, and English measure, is

$$V = \frac{307\,(\sqrt{d}-0,1)}{\sqrt{s}-L\sqrt{s+16}} - 0,3(\sqrt{d}-0,1)$$

And in French measure

$$V = \frac{297\,(\sqrt{d}-0,1)}{\sqrt{s}-L\sqrt{s+1,6}} - 0,3\,(\sqrt{d}-0,1)$$

The following table contains the real experiments from which this formula was deduced, and the comparison of the real velocities with the velocities computed by the formula. It consists of two principal sets of experiments. The first are those made on the motion of water in pipes. The second are experiments made on open canals and rivers. In the first set, column 1st contains the number of the experiment; 2d, the length of the tube; 3d, the height of the reservoir; 4th, the values of S, deduced from column second and third; 5th, gives the observed velocities; and 6th, the velocities calculated by the formula.

In the second set, column 2d gives the area of the section of the channel; 3d, the border of the canal or circumference of the section, deducting the horizontal width, which sustains no friction; 4th, the square root $\sqrt{d}$ of the hydraulic mean depth; 5th, the denominator S of the slope; 6th, the observed mean velocities; and 7th, the mean velocities by the formula. In the last ten experiments on large canals and a natural river the 6th column gives the observed velocities at the surface.

SET I.—EXPERIMENTS ON PIPES.

Experiments by Chevalier DE BUAT.

<table>
<tr><th>No</th><th>Length of Pipe.</th><th>Height of Reservoir.</th><th>Values of s.</th><th>Velocities observed.</th><th>Velocities calculated.</th></tr>
<tr><td colspan="6"><i>Vertical Tube ⅔ of a Line in Diameter and</i>
$\sqrt{d} = 0,117851.$</td></tr>
<tr><td></td><td>Inch.</td><td>Inch.</td><td>Inch.</td><td>Inch.</td><td>Inch.</td></tr>
<tr><td>1</td><td>12</td><td>16,166</td><td>0,75636</td><td>11,704</td><td>12,006</td></tr>
<tr><td>2</td><td>12</td><td>13,125</td><td>0,9307</td><td>9,753</td><td>10,576</td></tr>
<tr><td colspan="6"><i>Vertical Pipe 1½ Lines Diameter, and</i>
$\sqrt{d} = 0,176776$ <i>Inch.</i></td></tr>
<tr><td>3</td><td>34,166</td><td>42,166</td><td>0,9062</td><td>45,468</td><td>46,210</td></tr>
<tr><td>4</td><td>Do.</td><td>38,333</td><td>0,9951</td><td>43,156</td><td>43,721</td></tr>
<tr><td>5</td><td>Do.</td><td>36,666</td><td>1,0396</td><td>42,385</td><td>42,612</td></tr>
<tr><td>6</td><td>Do.</td><td>35,333</td><td>1,0781</td><td>41,614</td><td>41,714</td></tr>
<tr><td>7</td><td>Do.</td><td>14,583</td><td>2,5838</td><td>26,202</td><td>25,523</td></tr>
<tr><td>8</td><td>Do.</td><td>9,292</td><td>4,0367</td><td>21,064</td><td>19,882</td></tr>
<tr><td>9</td><td>Do.</td><td>5,292</td><td>7,036</td><td>14,642</td><td>14,447</td></tr>
<tr><td>10</td><td>Do.</td><td>2,083</td><td>17,6378</td><td>7,320</td><td>2,35</td></tr>
</table>

Vertical Pipe, 2 lines Diameter, and $\sqrt{d} = 0,204124$.

No.	Length of Pipe.	Height of Reservoir.	Values of s	Velocities observed.	Velocities calculated.
	Inch.	Inch.	Inch.	Inch.	Inch.
11	36,25	51,250	0,85451	64,373	64,945
12	Do.	45,250	0,96338	59,605	60,428
13	Do.	41,916	1,03808	57,220	57,838
14	Do.	38,750	1,12047	54,186	55,321

Same Pipe with a slope of $\frac{1}{1,3024}$.

15	36,25	33,500	1,29174	51,151	50,983

Same Pipe horizontal.

16	36,25	15,292	2,7901	33,378	33,167
17	Do.	8,875	4,76076	25,430	24,553
18	Do.	5,292	7,89587	19,940	18,313
19	Do.	2,042	20,01637	10,620	10,492

Vertical Pipe $2\frac{9}{10}$ *Lines Diameter, and* $\sqrt{d} = 0,245798$

20	36,25	53,250	0,95235	85,769	85,201
21	Do.	50,250	1,00642	82,471	82,461
22	Do.	48,333	1,0444	81,646	80,698
23	Do.	48,333	1,0444	79,948	
24	Do.	47,916	1,0529	81,027	80,318
25	Do.	44,750	1,1241	76,079	77,318
26	Do.	41,250	1,2157	73,811	73,904

The same pipe with the slope, $\frac{1}{1,3024}$

27	36,25	37,5	1,3323	70,822	70,138

The same Pipe horizontal.

No.	Length of Pipe.	Height of Reservoir.	Values of *s*.	Velocities observed.	Velocities calculated.
	Inch.	Inch.	Inch.	Inch.	Inch.
28	36,25	20,166	2,4303	51,956	50,140
29	Do.	9,083	5,2686	33,577	32,442
30	Do.	7,361	6,4504	28,658	28,801
31	Do.	5,	9,3573	23,401	23,195
32	Do.	4,916	9,5097	22,989	22,974
33	Do.	4,833	9,6652	22,679	22,754
34	Do.	3,708	12,4624	19,587	19,550
35	Do.	2,713	16,3135	16,631	16,324
36	Do.	2,083	21,6639	14,295	14,008
37	Do.	1,625	27,5102	12,680	12,115
38	Do.	0,833	52,3427	7,577	8,215

Pipes sensibly horizontal $\sqrt{d} = 0{,}5$, *or* 1 *inch diameter.*

39	117	36	5,6503	84,945	85,524
40	117	26,666	7,48	71,301	72,617
41	138,5	20,950	10,3215	58,808	60,034
42	117	18	10,7880	58,310	58,472
43	138,5	6	33,1962	29,341	29,663
44	737	23,7	33,6658	28,669	29,412
45	Do.	14,6	54,2634	21,856	22,056
46	Do.	13,7	57,7772	20,970	21,240
47	Do.	12,32	64,1573	19,991	19,950
48	Do.	8,96	87,8679	16,625	16,543
49	Do.	8,96		16,284	
50	Do.	7,780	101,0309	15,112	15,232
51	Do.	5,93	132,1617	13,315	13,005
52	Do.	4,2	186,0037	10,671	10,656
53	Do.	4,2		10,441	
54	138,5	0,7	257,8863	8,689	8,824
55	737	0,5	1540,75	3,623	3,218
56	737	0,15	5113,42	1,589	1,647

EXPERIMENTS BY THE ABBE BOSSUT.

Horizontal pipe 1 *inch diameter* $\sqrt{d} = 0{,}5$.

57	600	12	54,5966	22,282	21,975
58	600	4	161,312	12,223	11,756

Horizontal pipe 1⅓ *inch diameter* $\sqrt{d} = 0,5774$.

No.	Length of Pipe.	Height of Reservoir.	Values of *s*.	Velocities observed.	Velocities calculated.
	Inch.	Inch.	Inch.	Inch.	Inch.
59	360	24	19,0781	48,534	49,515
60	720	24	33,6166	34,473	35,130
61	360	12	37,0828	33,160	33,106
62	1080	24	48,3542	28,075	28,211
63	1440	24	63,1806	24,004	24,083
64	720	12	66,3020	23,360	23,345
65	1800	24	78,0532	21,832	21,182
66	2160	24	92,9474	18,896	19,096
67	1080	12	95,8756	18,943	18,749
68	1440	12	125,6007	16,128	15,991
69	1800	12	155,4015	14,066	14,119
70	2160	12	185,2487	12,560	12,750

Horizontal pipe 2,01 *inch diameter* $\sqrt{d} = 0,708946$.

71	360	24	21,4709	58,903	58,803
72	720	24	35,8082	43,	43,136
73	360	12	41,2759	40,322	39,587
74	1080	24	50,4119	35,765	35,096
75	1440	24	65,1448	30,896	30,096
76	720	12	70,1426	29,215	28,796
77	1800	24	79,8487	27,470	26,639
78	2160	24	94,7901	27,731	24,079
79	1080	12	99,4979	23,806	23,400
80	1440	12	129,0727	20,707	20,076
81	1800	12	158,7512	18,304	17,788
82	2160	12	188,5179	16,377	16,097

MR COUPLET'S EXPERIMENTS AT VERSAILLES.

Pipe 5 *inches diameter* $\sqrt{d} = 1,11803$.

83	84210	25	3378,26	5,323	5,287
84	Do.	24	3518,98	5,213	5,168
85	Do.	21,083	4005,66	4,806	4,807
86	Do.	16,750	5041,61	4,127	4,225
87	Do.	11,333	7450,42	3,154	3,388
88	Do.	5,583	15119,96	2,011	2,254

Pipe 18 *inches diameter* $\sqrt{d} = 2,12132$

89	43200	145,083	304,973	39,159	40,510

SET II.—EXPERIMENTS WITH A WOODEN CANAL.

No.	Section of Canal.	Border of Canal.	Values of $\sqrt{d}$.	Values of s.	Mean Velocity observed.	Mean Velocity calculated.
			Trapezium Canal.			
	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
90	18,84	13,06	1,20107	212	27,51	27,19
91	50,60	29,50	1,3096	212	28,92	29,88
92	83,43	26,	1,7913	412	27,14	28,55
93	27,20	15,31	1,3329	427	18,28	20,39
94	39,36	18,13	1,4734	427	20,30	22,71
95	50,44	20,37	1,5736	427	22,37	24,37
96	56,43	21,50	1,6201	427	23,54	25,14
97	98,74	28,25	1,8696	432	28,29	29,06
98	100,74	28,53	1,8791	432	28,52	29,23
99	119,58	31,06	1,9622	432	30,16	30,60
100	126,20	31,91	1,9887	432	31,58	31,03
101	130,71	32,47	2,0064	432	31,89	31,32
102	135,32	33,03	2,0241	432	32,52	31,61
103	20,83	13,62	1,2367	1728	8,94	8,58
104	34,37	17,	1,4219	1728	9,71	9,98
105	36,77	17,56	1,4471	1728	11,45	10,17
106	42,01	18,69	1,4992	1728	12,34	10,53
			Rectangular Canal.			
107	34,50	21,25	1,27418	458	20,24	18,66
108	86,25	27,25	1,77908	458	28,29	26,69
109	34,50	21,25	1,27418	929	13,56	12,53
110	35,22	21,33	1,28499	1412	9,20	10,01
111	51,75	23,25	1,49191	1412	12,10	11,76
112	76,19	26,08	1,70921	1412	14,17	13,59
113	105,78	29,17	1,90427	1412	15,55	15,24
114	69,	25,25	1,65308	9288	4,59	4,56
115	155,25	35,25	2,09868	9288	5,70	5,86

SET III.—EXPERIMENTS ON THE CANAL OF JARD.

No.	Section of Canal.	Border of Canal.	Values of $\sqrt{d}$.	Values of s.	Velocity observed at Surface.	Velocity calculat.
116	16252	402	6,3583	8919	17,42	18,77
117	11905	366	5,70320	11520	12,17	14,52
118	10475	360	5,3942	15360	15,74	11,61
119	7858	340	4,8074	21827	9,61	8,38
120	7376	337	4,6784	27648	7,79	7,07
121	6125	324	4,3475	27648	7,27	6,55

Experiments on the River Haine.

No.	Section of River.	Border of River.	Values of $\sqrt{d}$.	Values of s.	Velocity at Surface.	Velocity (mean) calculat.
122	31498	569	7,43974	6048	35,11	27,62
123	38838	601	8,03879	6413	31,77	28,76
124	30905	568	7,37632	32951	13,61	10,08
125	39639	604	8,10108	35723	15,96	10,53

This comparison must be acknowledged to be most satisfactory, and shows the great penetration and address of the author, in so successfully sifting and appreciating the share which each co-operating circumstance has had in producing the very intricate and complicated effect. It adds some weight to the principles on which he has proceeded in this analysis of the mechanism of hydraulic motion, and must give us great confidence in a theory so fairly established on a very copious induction. The author offers it only as a rational and well-founded probability. To this character it is certainly entitled; for the suppositions made in it are agreeable to the most distinct notions we can form of these internal motions. And it must always be remembered, that the investigation of the formula, although it be rendered somewhat more perspicuous by thus having recourse to those notions, has no dependence on the truth of the principles. For it is, in fact, nothing but a classification of experiments, which are grouped together by some one circumstance of slope, velocity, form of section, &c. in order to discover the law of the changes which are induced by a variation of the circumstances which do not resemble. The procedure was precisely similar to that of the astronomer when he deduces the elements of an orbit from a multitude of observations. This was the task of M. de Buat; and he candidly and modestly informs us, that the finding out analytical forms of expression which would exhibit these changes was the work of Mr Benzech de St Honoré, a young officer of engineers, and his colleague in the experimental course. It does honour to his skill and address; and we think the whole both a pretty and instructive specimen of the method of discovering the laws of nature in the midst of complicated phenomena. Daniel Bernoulli first gave the rules of this method, and they have been greatly improved by Lambert, Condorcet, and De la Grange. Mr Coulomb has given some excellent examples of their application to the discovery of the laws of friction, of magnetical and electrical attraction, &c. But this present work is the most perspicuous and familiar of them all. It is the empirical method of generalizing natural phenomena, and of deducing general rules, of which we can give no other demonstration but that they are faithful representations of matters of fact. We hope that others, encouraged by the success of M. de Buat, will follow this example, where public utility is preferred to a display of mathematical knowledge.

Although the author may not have hit upon the precise *modus operandi*, we agree with him in thinking that nature seems to act in a way not unlike what is here supposed. At any rate, the range of experiments is so extensive, and so multifarious, that few cases can occur which are not included among them. The experiments will always retain their value (as we presume that they are faithfully narrated), whatever may become of the theory; and we are confident that the formula will give an answer to any question to which it may be applicable infinitely preferable to the vague guess of the most sagacious and experienced engineer.

We must however observe, that as the experiments on pipes were all made with scrupulous care in the contrivance and execution of the apparatus, excepting only those of Mr Couplet on the main pipes at Versailles, we may presume, that the formula gives the greatest velocities which can be expected. In ordinary works, where joints are rough or leaky, where drops of solder hang in the inside, where cocks intervene with deficient water-ways, where pipes have awkward bendings, contractions, or enlargements, and where they may contain sand or air, we should reckon on a smaller velocity than what results from our calculation; and we presume that an undertaker may with confidence promise ⅘ of this quantity without any risk of disappointing his employer. We imagine that the actual performance of canals will be much nearer to the formula.

We have made inquiry after works of this kind executed in Britain, that we might compare them with the formula. But all our canals are locked and without motion; and we have only learned by an accidental information from Mr Watt, that a canal in his neighbourhood, which is 18 feet wide at the surface, and seven feet at the bottom, and four feet deep, and has a slope of one inch in a quarter of a mile, runs with the velocity of 17 inches per second at the surface, 10 at the bottom, and 14 in the middle. If we compute the motion of this canal by our formula, we shall find the mean velocity to be 13⅓.

No river in the world has had its motions so mush scrutinized as the Po about the end of the last century. It had been a subject of 100 years continual litigation between the inhabitants of the Bolognese and the Ferrarese, whether the waters of the Rheno should be thrown into the Tronco de Venezia or Po Grande. This occasioned very numerous measures to be taken of its sections and declivity, and the quantities of water which it contained in its different states of fulness. But, unfortunately, the long-established methods of measuring waters, which were in force in Lombardy, made no account of the velocity, and not all the entreaties of Castelli, Grandi, and other moderns, could prevail on the visitors in this process to deviate from the established methods. We have therefore no minute accounts of its velocity, though there are many rough estimates to be met with in that valuable collection published at Florence in 1723, of the writings on the motion of rivers. From them we have extracted the *only precise observations* which are to be found in the whole work.

The Po Grande receives no river from Stellata to the sea, and its slope in that interval is found most surprisingly uniform, namely, six inches in the mile (reduced to English measure). The breadth in its great freshes is 759 feet at Lago Scuro, with a very uniform depth of 31 feet. In its lowest state (in which it is called *Po Magra*), its breadth is not less than 700, and its depth about 10½.

The Rheno has a uniform declivity from the Ponte Emilio to Vigarano of 15 inches per mile. Its breadth in its greatest freshes is 189 feet, and its depth 9.

Signor Corrade, in his report, says, that in the state of the great freshes the velocity of the Rheno is most exactly $\frac{4}{5}$ of that of the Po.

Grandi says, that a great fresh in the Rheno employs 12 hours (by many observations of his own) to come from Ponte Emilio to Vigarano, which is 30 miles. This is a velocity of 44 inches per second. And, by Corrade's proportion, the velocity of the Po Grande must be 55 inches per second.

Montanari's observation gives the Po Magra a velocity of 31 inches per second.

Let us compare these velocities with the velocities calculated by Buat's formula.

The hydraulic mean depths d and D of the Rheno and Po in the great freshes deduced from the above measures, are 98,6 and 344 inches; and their slopes s and S are $\frac{1}{4224}$ and $\frac{1}{10560}$. This will give, $\frac{307(\sqrt{D}-0,1)}{\sqrt{S}-L\sqrt{S+1,6}}-0,3$ $(\sqrt{D}-0,1)=52,176$ inches and $\frac{307(\sqrt{d}-0,1)}{\sqrt{s}-L\sqrt{s+1,6}}-0,3$ $(\sqrt{d}-0,1)=46,727$ inches.

These results differ very little from the velocities abovementioned. And if the velocity corresponding to a depth of 31 feet be deduced from that observed by Montanari in the Po Magra 10 feet deep, on the supposition that they are in the proportion of $\sqrt{d}$, it will be found to be about 53½ inches per second.

This comparison is therefore highly to the credit of the theory, and would have been very agreeable to M. de Buat, had he known it, as we hope it is to our readers.

We have collected many accounts of water-pipes, and made the comparisons, and we flatter ourselves that these have enabled us to improve the theory. They shall appear in their proper place; and we may just observe here, that the two-inch pipe, which we formerly spoke of as conveying the water to Dunbar, should have yielded only 25⅔ Scotch pints per minute by the formula, instead of 27; a small error.

We have, therefore, no hesitation in saying, that this single formula of the uniform motion of water is one of the most valuable presents which natural science and the arts have received during the course of this century.

We hoped to have made this fortunate investigation of the chevalier de Buat still more acceptable to our readers by another table, which should contain the values of $\frac{307}{\sqrt{s} - \mathrm{L}\sqrt{s + 1{,}6}}$ ready calculated for every declivity that can occur in water-pipes, canals, or rivers. Aided by this, which supersedes the only difficult part of the computation, a person could calculate the velocity for any proposed case in less than two minutes. But we have not been able to get it ready for its appearance in this article, but we shall not fail to give it when we resume the subject in the article WATER-WORKS; and we hope even to give its results on a scale which may be carried in the pocket, and will enable the unlearned practitioner to solve any question with accuracy in half a minute.

WE have now established in some measure a THEORY OF HYDRAULICS, by exhibiting a general theorem which expresses the relation of the chief circumstances of all such motions as have attained a state of permanency, in so far as this depends on the magnitude, form, and slope of the channel. This permanency we have expressed by the term TRAIN, saying, that the stream is *in train*.

We proceed to consider the subordinate circumstances contained in this theorem; such as, 1*st*, The forms which nature or art may give to the bed of a running stream, and the manner of expressing this form in our theorem. 2*d*, The gradations of the velocity, by which it decreases in the different filaments, from the axis or most rapid filament to the border; and the connexion of this with the mean velocity, which is expressed by our formula. 3*d*, Having acquired some distinct notions of this, we shall be able to see the manner in which undisturbed nature works in forming the beds of our rivers, the forms which she affects, and which we must imitate in all their local modifications, if we would secure that permanency which is the evident aim of all her operations. We shall here learn the mutual action of the current and its bed, and the circumstances which ensure the stability of both. These we may call the *regimen* or the *conservation* of the stream, and may say that it is *in regimen*, or *in conservation*. This has a relation, not to the dimensions and the slope alone, or to the accelerating force and the resistance arising from mere inertia; it respects immediately the tenacity of the bed, and is different from *the train*.

4*th*, These pieces of information will explain the deviation of rivers from the rectilineal course; the resistance occasioned by these deviations; and the circumstances on which the regimen of a winding stream depends.

SECT. I.—*Of the Forms of the Channel.*

THE numerator of the fraction which expresses the velocity of a river in train has $\sqrt{d}$ for one of its factors. That form, therefore, is most favourable to the motion, which gives the greatest value to what we have called the hydraulic mean depth *d*. This is the prerogative of the semicircle, and here *d* is equal to half the radius; and all other figures of the same area are the more favourable, as they approach nearer to a semicircle. This is the form, therefore, of all conduit-pipes, and should be taken for aqueducts which are built of masonry. Ease and accuracy of execution, however, have made engineers prefer a rectangular form; but neither of these will do for a channel formed out of the ground. We shall soon see that the semicircle is incompatible with a regimen; and, if we proceed through the regular polygons, we shall find that the half hexagon is the only one which has any pretensions to a regimen; yet experience shows us, that even its banks are too steep for almost any soil. A dry earthen bank, not bound together by grass roots, will hardly stand with a slope of 45 degrees; and a canal which conveys running waters will not stand with this slope. Banks whose base is to their height as four to three will stand very well in moist soils, and this is a slope very usually given. This form is even affected in the spontaneous operations of nature, in the channels which she digs for the rills and rivulets in the higher and steeper grounds.

This form has some mathematical and mechanical properties which entitle it to some further notice. Let ABEC (Fig. 12.) be such a trapezium, and AHGC the rectangle of equal width and depth. Bisect HB and EG by the verticals FD and KI, and draw the verticals *b* B, *e* E. Because AH : HB = 3 : 4, we have AB = 5, and BD = 2, and FD = 3, and BD + DF = BA. From these premises it follows, that the trapezium ABEC has the same area with the rectangle; for HB being bisected in D, the triangles ACF, BCD are equal. Also the border ABEC, which is touched by the passing stream, is equal to FDIK. Therefore the mean depth, which is the quotient of the area divided by the border, is the same in both; and this is the case, whatever is the width BE at the bottom, or even though there be no rectangle such as *b* B E *e* interposed between the slant sides.

Of all rectangles, that whose breadth is twice the height, or which is half of a square, gives the greatest mean depth. If, therefore, FK be double of FD, the trapezium ABEC, which has the same area, will have the largest mean depth of any such trapezium, and will be the best form of a channel for conveying running waters. In this case, we have AC = 10, AH = 3, and BE = 2. Or we may say that the best form is a trapezium, whose bottom width is $\frac{2}{3}$ of the depth, and whose extreme width is $\frac{10}{3}$. This form approaches very near to that which the torrents in the hills naturally dig for themselves in uniform ground, where their action is not checked by stones which they lay bare, or which they deposit in their course. This shows us, and it will be fully confirmed by and by, that the channel of a river is not a fortuitous thing, but has a relation to the consistency of the soil and velocity of the stream.

A rectangle, whose breadth is $\frac{4}{3}$ of the depth of water, will therefore have the same mean depth with a triangle whose surface wide is $\frac{8}{3}$ of its vertical depth; for this is the dimensions when the rectangle *b* B E *e* is taken away.

Let A be the area of the section of any channel, w its width (when rectangular), and h its depth of water. Then what we have called its mean depth, or d, will be $\frac{A}{w+2h} = \frac{w\,h}{w+2\,h}$. Or if q expresses the ratio of the width to the depth of a rectangular bed; that is, if $q = \frac{w}{h}$, we have a very simple and ready expression for the mean depth, either from the width or depth. For $d = \frac{w}{q+2}$, or $d = \frac{q\,h}{q+2}$.

Therefore, if the depth were infinite, and the width finite, we should have $d = \frac{w}{2}$; or if the width be infinite, and the depth finite, we have $d = h$. And these are the limits of the values of d; and therefore, in rivers whose width is always great in comparison of the depth, we may without much error take their real depth for their hydraulic mean depth. Hence we derive a rule of easy recollection, and which will at all times give us a very near estimate of the velocity and expense of a running stream, viz. that *the velocities are nearly as the square roots of the depths.* We find this confirmed by many experiments of Michelotti.

Also, when we are allowed to suppose this ratio of the velocities and depths, that is, in a rectangular canal of great breadth and small depth, we shall have the quantities discharged nearly in the proportion of the cubes of the velocities. For the quantity discharged d is as the velocity and area jointly, that is, as the height and velocity jointly, because when the width is the same, the area is as the height. Therefore, we have $d \doteqdot h\,v$.—But, by the above remark, $h \doteqdot v^2$. Therefore, $d \doteqdot v^3$; and this is confirmed by the experiments of Bossut, vol. ii. 296.—Also, because d is as $v\,h$, when w is constant, and by the above remark (allowable when w is very great in proportion to h) v is as $\sqrt{h}$, we have d as $h\sqrt{h}$, or $h^{\frac{3}{2}}$, or the squares of the discharges proportional to the cubes of the heights in rectangular beds, and in their corresponding trapeziums.

1. Knowing the mean depth and the proportion of the width and real depth, we can determine the dimensions of the bed, and we have $w = q\,d + 2d$, and $h = d + \frac{2d}{q}$.

2. If we know the area and mean depth, we can in like manner find the dimensions, that is, w and h; for $A = w\,h$, and $d = \frac{w\,h}{w+2h}$; therefore $w = \pm\sqrt{\frac{A^2}{4d^2} - 2A} + \frac{A}{2d}$

3. If d be known, and one of the dimensions be

given, we can find the other; for $d = \frac{w\,h}{w + 2\,h}$ gives $w = \frac{2\,h\,d}{h - d}$, and $h = \frac{w\,d}{w - 2\,d}$.

4. If the velocity V and the slope S for a river in train be given, we can find the mean depth; for $V = \left(\frac{297}{\sqrt{S} - L\sqrt{S + 1,6}} - 0,3\right)(\sqrt{d} - 0,1)$. Whence we deduce $\sqrt{d} - 0,1 = \frac{V}{\frac{297}{\sqrt{S} - L\sqrt{S+1,6}} - 0,3}$, and $\sqrt{d}$ = to this quantity $+ 0,1$.

5. We can deduce the slope which will put in train a river whose channel has given dimensions. We make $\frac{297\,(\sqrt{d} - 0.\,1)}{V + 0,\,3\,(\sqrt{d} - 0,\,1)} = \sqrt{S}$. This should be $= \sqrt{S} - L\sqrt{S + 1,\,6}$, which we correct by trials, which will be exemplified when we apply these doctrines to practice.

Having thus established the relation between the different circumstances of the form of the channel to our general formula, we proceed to consider,

SECT. 2.—*The Gradations of Velocity from the Middle of the Stream to the Sides.*

THE knowledge of this is necessary for understanding the regimen of a river; for it is the velocity of the filaments in contact with the bed which produces any change in it, and occasions any preference of one to another, in respect of regimen or stability. Did these circumstances not operate, the water, true to the laws of hydraulics, and confined within the bounds which have been assigned them, would neither enlarge nor diminish the area of the channel. But this is all that we can promise of waters perfectly clear, running in pipes or hewn channels. But rivers, brooks, and smaller streams, carry along waters loaded with mud or sand, which they deposite wherever their velocity is checked; and they tear up, on the other hand, the materials of the channel wherever their velocity is sufficiently great. Nature, indeed, aims continually at an equilibrium, and works without ceasing to perpetuate her own performances, by establishing an equality of action and reaction, and proportioning the forms and direction of the motions to her agents, and to local circumstances. Her work is slow but unceasing; and what she cannot accomplish in a year she will do in a century. The beds of our rivers have acquired some stability, because they are the labour of ages; and it is to time that we owe those deep and wide valleys which receive and confine our rivers in channels, which are now consolidated, and with slopes which have been gradually moderated, so that they no longer either ravage our habitations or confound our boundaries. Art may imitate nature, and by directing her operations (which she still carries on according to her own imprescriptible laws) according to our views, we can hasten her progress, and accomplish our purpose, during the short period of human life. But we can do this only by studying the unalterable laws of mechanism. These are presented to us by spontaneous nature. Frequently we remain ignorant of their foundation: but it is not necessary for the prosperity of the subject that he have the talents of the senator; he can profit by the statute without understanding its grounds. It is so in the present instance. We have not as yet been able to infer the law of retardation observed in the filaments of a running stream from any sound mechanical principle. The problem, however, does not appear beyond our powers, if we assume, with Sir Isaac Newton, that the velocity of any particular filament is the arithmetical mean between those of the filaments immediately adjoining. We may be assured, that the filament in the axis of an inclined cylindrical tube, of which the current is in train, moves the fastest, and that all those in the same circumference round it are moving with one velocity, and that the slowest are those which glide along the pipe. We may affirm the same thing of the motions in a semi-cylindrical inclined channel conveying an open stream. But even in these we have not yet demonstrated the ratio between the extreme velocities, nor in the different circles. This must be decided experimentally.

And here we are under great obligations to M. de Buat. He has compared the velocity in the axis of a prodigious number and variety of streams, differing in size, form, slope, and velocity, and has computed in them all the mean velocity, by measuring the quantities of water discharged in a given time. His method of measuring the bottom velocity was simple and just. He threw in a gooseberry, as nearly as possible, of the same specific gravity with the water. It was carried along the bottom almost without touching it. See RESISTANCE OF FLUIDS in this volume.

He discovered the following laws: 1. In small velocities the velocity in the axis is to that at the bottom in a ratio of considerable inequality. 2. This ratio diminishes as the velocity increases, and in very great velocities, approaches to the ratio of equality. 3. What was most remarkable was, that neither the magnitude of the channel, nor its slope, had any influence on changing this proportion, while the mean velocity remained the same. Nay, though the stream ran on a channel covered with pebbles or coarse sand, no difference worth minding was to be observed from the velocity over a polished channel. 4. And if the velocity in the axis is constant, the velocity at the bottom is also constant, and is not affected by the depth of water or magnitude of the stream. In some experiments the depth was thrice the width, and in others the width was thrice the depth. This changed the proportion of the magnitude of the section to the magnitude of the rubbing part, but made no change on the ratio of the velocities. This is a thing which no theory could point out.

Another most important fact was also the result of his observation, viz. that *the mean velocity in any pipe or open stream is the arithmetical mean between the velocity in the axis and the velocity at the sides of a pipe or bottom of an open stream.* We have already observed, that the ratio of the velocity in the axis to the velocity at the bottom diminished as the mean velocity increased. This variation he was enabled to express in a very simple manner, so as to be easily remembered, and to enable us to tell any one of them by observing another.

If we take unity from the square-root of the superficial velocity, expressed in inches, the square of the remainder is the velocity at the bottom; and the mean velocity is the half sum of these two. Thus, if the velocity in the middle of the stream be 25 inches per second, its square root is five; from which, if we take unity, there remains four. The square of this, or 16, is the velocity at the bottom, and $\frac{25+16}{2}$, or $20\frac{1}{2}$, is the mean velocity.

This is a very curious and most useful piece of information. The velocity in the middle of the stream is the easiest measured of all, by any light small body floating down it; and the mean velocity is the one which regulates the train, the discharge, the effect on machines, and all the most important consequences.

We may express this by a formula of most easy recollection. Let V be the mean velocity, v the velocity in the axis, and u the velocity at the bottom; we have $u = \overline{\sqrt{v}-1}^{\,2}$, and $V = \frac{v+u}{2}$.

Also $v = (\sqrt{V-\frac{1}{4}}+\frac{1}{2})^2$, and $v = (\sqrt{u}+1)^2$.
$V = (\sqrt{v}-\frac{1}{2})^2+\frac{1}{4}$, and $V = (\sqrt{u}+\frac{1}{2})^2+\frac{1}{4}$.
$u = (\sqrt{v}-1)^2$ and $u = (\sqrt{V-\frac{1}{4}}-\frac{1}{2})^2$.

Also $v-u = 2\sqrt{V-\frac{1}{4}}$ and $v - V, = V - u, = \sqrt{V-\frac{1}{4}}$: that is, the difference between these velocities increases in the ratio of the square roots of the mean velocities diminished by a small constant quantity.

This may perhaps give the mathematicians some help in ascertaining the law of degradation from the axis to the sides. Thus, in a cylindrical pipe, we may conceive the current as consisting of an infinite number of cylindrical shells sliding within each other like the draw-tubes of a spy-glass. Each of these is in equilibrio, or as much accelerated by the one within it as it is retarded by the one without; therefore as the *momentum* of each diminishes in the proportion of its diameter (the thickness being supposed the same in all), the velocity of separation must increase by a certain law from the sides to the axis. The magnitude of the small constant quantity here spoken of seems to fix this law.

The place of the mean velocity could not be discovered with any precision. In moderate velocities it was not more than one-fourth or one-fifth of the depth distant from the bottom. In very great velocities it was sensibly higher, but never in the middle of the depth.

The knowledge of these three velocities is of great importance. The superficial velocity is easily observed; hence the mean velocity is easily computed. This multiplied by the section gives the expense; and if we also measure the expanded border, and then obtain the mean depth (or $\surd d$), we can, by the formula of uniform motion, deduce the slope; or, knowing the slope, we can deduce any of the other circumstances.

The following table of these three velocities will save the trouble of calculation in one of the most frequent questions of hydraulics.

Velocity in Inches.			Velocity in Inches.		
Surface.	Bottom.	Mean.	Surface.	Bottom.	Mean.
1	0,000	0,5	51	37,712	44,356
2	0,172	1,081	52	38,564	45,282
3	0,537	1,768	53	39,438	46,219
4	1,	2,5	54	40,294	47,147
5	1,526	3,263	55	41,165	48,082
6	2,1	4,050	56	42,016	49,008
7	2,708	4,854	57	42,968	49,984
8	3,342	5,67	58	43,771	50,886
9	4,	6,5	59	44,636	51,818
10	4,674	7,337	60	45,509	52,754
11	5,369	8,184	61	46,376	53,688
12	6,071	9,036	62	47,250	54,625
13	6,786	9,893	63	48,136	55,568
14	7,517	10,756	64	49,	56,5
15	8,254	11,627	65	49,872	57,436
16	9,	12,5	66	50,751	58,376
17	9,753	13,376	67	51,639	59,219
18	10,463	14,231	68	52,505	60,252
19	11,283	15,141	69	53,392	61,196
20	12,055	16,027	70	54,273	62,136
21	12,674	16,837	71	55,146	63,073
22	13,616	17,808	72	56,025	64,012
23	14,402	18,701	73	56,862	64,931
24	15,194	19,597	74	57,790	65,895
25	16,	20,5	75	58,687	66,843
26	16,802	21,401	76	59,568	67,784
27	17,606	22,303	77	60,451	68,725
28	18,421	23,210	78	61,340	69,670
29	19,228	24,114	79	62,209	70,605
30	20,044	25,022	80	63,107	71,553
31	20,857	25,924	81	64,	72,5
32	21,678	26,839	82	64,883	73,441
33	22,506	27,753	83	65,780	74,390
34	23,339	28,660	84	66,651	75,325
35	24,167	29,583	85	67,568	76,284
36	25,	30,5	86	68,459	77,229
37	25,827	31,413	87	69,339	78,169
38	26,667	32,333	88	70,224	79,112
39	27,51	33,255	89	71,132	80,066
40	28,345	34,172	90	72,012	81,006
41	29,192	35,096	91	72,915	81,957
42	30,030	36,015	92	73,788	82,894
43	30,880	36,940	93	74,719	83,859
44	31,742	37,871	94	75,603	84,801
45	32,581	38,790	95	76,51	85,755
46	33,432	39,716	96	77,370	86,685
47	34,293	40,646	97	78,305	87,652
48	35,151	41,570	98	79,192	88,596
49	36,	42,5	99	80,120	89,56
50	36,857	43,428	100	81,	90·5

The knowledge of the velocity at the bottom is of the greatest use for enabling us to judge of the action of the stream on its bed; and we shall now make some observations on this particular.

Every kind of soil has a certain velocity consistent with the stability of the channel. A greater velocity would enable the waters to tear it up, and a smaller velocity would permit the deposition of more moveable materials from above. It is not enough, then, for the stability of a river, that the accelerating forces are so adjusted to the size and figure of its channel that the current may be in train: it must also be in equilibrio with the tenacity of the channel.

We learn from observation, that a velocity of three inches per second at the bottom will just begin to work upon fine clay fit for pottery, and however firm and compact it may be, it will tear it up. Yet no beds are more stable than clay when the velocities do not exceed this: for the water soon takes away the impalpable particles of the superficial clay, leaving the particles of sand sticking by their lower half in the rest of the clay, which they now protect, making a very permanent bottom, if the stream does not bring down gravel or coarse sand, which will rub off this very thin crust, and allow another layer to be worn off; a velocity of six inches will lift fine sand; eight inches will lift sand as coarse as linseed; 12 inches will sweep along fine gravel; 24 inches will roll along rounded pebbles an inch diameter; and it requires three feet per second at the bottom to sweep along shivery angular stones of the size of an egg.

The manner in which unwearied nature carries on some of these operations is curious, and deserves to be noticed a little. All must recollect the narrow ridges or wrinkles which are left on the sand by a temporary fresh or stream. They are observed to lie across the stream, and each ridge consists of a steep face AD, BF (Fig. 24.) which looks down the stream, and a gentler slope DB, FC, which connects this with the next ridge. As the stream comes over the first steep AD, it is directed almost perpendicularly against the point E immediately below D, and thus it gets hold of a particle of coarse sand, which it could not have detached from the rest had it been moving parallel to the surface of it. It easily rolls up the gentle slope EB; arrived there, the particle tumbles over the ridge, and lies close at the bottom of it at F, where it is protected by the little eddy, which is formed in the very angle; other particles lying about E are treated in the same way, and, tumbling over the ridge B, cover the first particle, and now protect it effectually from any further disturbance. The same operation is going on at the bottom of each ridge. The brow or steep of the ridge gradually advances down the stream, and the whole set change their places, as represented by the dotted line *a d b f*; and after a certain time the particle which was deposited at F is found in an unprotected situation, as it was in E, and it now makes another step down the stream.

The Abbé Bossut found, that when the velocity of the stream was just sufficient for lifting the sand (and a small excess hindered this operation altogether) a ridge advanced about 20 feet in a day.

Since the current carries off the most moveable matters of the channel, it leaves the bottom covered with the remaining coarser sand, gravel, pebbles, and larger stones. To these are added many which come down the stream while it is more rapid, and also many which roll in from the sides as the banks wear away. All these form a bottom much more solid and immoveable than a bottom of the medium soil would have been. But this does not always maintain the channel in a permanent form; but frequently occasions great changes, by obliging the current, in the event of any sudden fresh or swell, to enlarge its bed, and even to change it altogether, by working to the right and to the left, since it cannot work downwards. It is generally from such accumulation of gravel and pebbles in the bottom of the bed that rivers change their channels.

It remains to ascertain, in absolute measures, the force which a current really exerts in attempting to drag along with it the materials of its channel; and which *will* produce this effect unless resisted by the inertia of these materials. It is therefore of practical importance to know this force.

Nor is it abstruse or difficult. For when a current is in train, the accelerating force is in equilibrio with the resistance, and is therefore its immediate measure. Now this accelerating force is precisely equal to the weight of the body of water in motion multiplied by the fraction which expresses the slope. The mean depth being equal to the quotient of the section divided by the border, the section is equal to the product of the mean depth multiplied by the border. Therefore, calling the border b, and the mean depth d, we have the section $= d b$. The body of water in motion is therefore $d b s$ (because s was the slant length of a part whose difference of elevation is 1), and the accelerating force is $d b s \times \frac{1}{s}$, or $d b$. But, if we would only consider this resistance as corresponding to an unit of the length of the channel, we must divide the quantity $d b$ by s, and the resistance is then $\frac{d b}{s}$. And if we would consider the resistance only for an unit of the border, we must divide this expression by b; and thus this resistance (taking an inch for the unit) will be expressed for one square inch of the bed by the weight of a bulk of water which has a square inch for its base, and $\frac{d}{s}$ for it height. And, lastly, if E be taken for any given superficial extent of the channel or bed, and F the obstruction, which we consider as a sort of friction, we shall have $F = \frac{E d}{s}$.

Thus, let it be required to determine in pounds the resistance or friction on a square yard of a channel whose current is in train, which is 10 feet wide, four feet deep, and has a slope of one foot in a mile. Here E is nine feet. Ten feet width and four feet depth give a section of 40 feet. The border is 18 feet. Therefore $d = \frac{40}{18} = 2{,}1111$, and s is 5280. Therefore the friction is the weight of a column of water whose base is nine feet, and height $\frac{2{,}1111}{5280}$, or nearly $3\frac{6}{10}$ ounces avoirdupois.

SECT. 3.—*Settlement of the Beds of Rivers.*

HE who looks with a careless eye at a map of this world, is apt to consider the rivers which ramble over its surface as a chance-medley disposition of the drainers which carry off the waters. But it will afford a most agreeable object to a considerate and contemplative mind, to take it up in this very simple light; and having considered the many ways in which the drenched surface might have been cleared of the superfluous waters, to attend particularly to the very way which nature has followed. In following the troubled waters of a mountain torrent, or the pure streams which trickle from their bases, till he sees them swallowed up in the ocean, and in attending to the many varieties in their motions, he will be delighted with observing how the simple laws of mechanism are made so fruitful in good consequences, both by modifying the motions of the waters themselves, and also by inducing new forms on the surface of the earth, fitted for re-acting on the waters, and producing these very modifications of their motions which render them so beneficial. The permanent beds of rivers are by no means fortuitous gutters hastily scooped out by dashing torrents; but both they and the valleys through which they flow are the patient but unceasing labours of nature, prompted by goodness and directed by wisdom.

Whether we trace a river from the torrents which collect the superfluous waters of heaven, or from the springs which discharge what would otherwise be condemned to perpetual inactivity, each feeder is but a little rill which could not ramble far from its scanty source among growing plants and absorbent earth, without being sucked up and evaporated, did it not meet with other rills in its course. When united they form a body of water still inconsiderable, but much more able, by its bulk, to overcome the little obstacles to its motion; and the rivulet then moves with greater speed, as we have now learned. At the same time, the surface exposed to evaporation and absorption is diminished by the union of the rills. Four equal rills have only the surface of two when united. Thus the portion which escapes arrestment, and travels downwards, is continually increasing. This is a happy adjustment to the other operations of nature. Were it otherwise, the lower and more valuable countries would be loaded with the passing waters in addition to their own surplus rains, and the immediate neighbourhood of the sea would be almost covered by the drains of the interior countries. But, fortunately, those passing waters occupy less room as they advance, and by this wise employment of the most simple means, not only are the superfluous waters drained off from our fertile fields, but the drains themselves become an useful part of the country by their magnitude. They become the habitation of a prodigious number of fishes, which share the Creator's bounty; and they become the means of mutual communication of all the blessings of cultivated society. The vague ramblings of the rivers scatter them over the face of the country, and bring them to every door. It is not even an indifferent circumstance, that they gather strength to cut out deep beds for themselves. By this means they cut open many springs. Without this, the produce of a heavy shower would make a swamp which would not dry up in many days. And it must be observed, that the same heat which is necessary for the growth of useful plants will produce a very copious evaporation. This must return in showers much too copious for immediate vegetation, and the overplus would be destructive. Is it not pleasant to contemplate this adjustment of the great operations of nature, so different from each other, that if chance alone directed the detail, it was almost an infinite odds that the earth would be uninhabitable?

But let us follow the waters in their operations, and note the face of the countries through which they flow: attending to the breadth, the depth, and the slope of the valleys, we shall be convinced that their present situation is extremely different from what it was in ancient days; and that the valleys themselves are the works of the rivers, or at least of waters which have descended from the heights, loaded with all the lighter matters which they were able to bring away with them. The rivers flow now in beds which have a considerable permanency; but this has been the work of ages. This has given stability, both by filling up and smoothing the valleys, and thus lessening the changing causes, and also by hardening the beds themselves, which are now covered with aquatic plants, and lined with the stones, gravel, and coarser sand, out of which all the lighter matters have been washed away.

The surface of the high grounds is undergoing a continual change; and the ground on which we now walk is by no means the same which was trodden by our remote ancestors. The showers from heaven carry down into the valleys, or sweep along by the torrents, a part of the soil which covers the heights and steeps. The torrents carry this soil into the brooks, and these deliver part of it into the great rivers, and these discharge into the sea this fertilizing fat of the earth, where it is swallowed up, and forever lost for the purposes of vegetation. Thus the hillocks lose of their height, the valleys are filled up, and the mountains are laid bare, and show their naked precipices, which formerly were covered over with a flesh and skin, but now look like the skeleton of this globe. The low countries, raised and nourished for some time by the substance of the high lands, will go in their turn to be buried in the ocean; and then the earth, reduced to a dreary flat, will become an immense uninhabitable mass. This catastrophe is far distant, because this globe is in its youth, but it is not the less certain; and the united labours of the human race could not long protract the term.

But, in the mean time, we can trace a beneficent purpose, and a nice adjustment of seemingly remote circumstances. The grounds near the sources of all our rivers are indeed gradually stripped of their most fertile ingredients. But had they retained them for ages, the sentient inhabitants of the earth, or at least the nobler animals, with man at their head, would not have derived much advantage from it. The general laws of nature produce changes in our atmosphere which must ever render these great elevations unfruitful. That genial warmth, which is equally necessary for the useful plant as for the animal which lives on it, is confined to the lower grounds. The earth, which on the top of mount Hæmus could only bring forth moss and dittany, when brought into the gardens of Spalatro, produced pot-herbs so luxuriant, that Dioclesian told his colleague Maximian that he had more pleasure in their cultivation than the Roman empire could confer. Thus nature not only provides us manure, but conveys it to our fields. She even keeps it safe in store for us till it shall be wanted. The tracts of country which are but newly inhabited by man, such as great part of America, and the newly-discovered regions of Terra Australis, are still almost occupied by marshes and lakes, or covered with impenetrable forests; and they would remain long enough in this state, if population, continually increasing, did not increase industry, and multiply the hands of cultivators along with their necessities. The Author of Nature was alone able to form the huge ridges of the mountains, to model the hillocks and the valleys, to mark out the courses of the great rivers, and give the first trace to every rivulet; but has left to man the task of draining his own habitation and the fields which are to support him, because this is a task not beyond his powers. It was therefore of immense advantage to him that those parts of the globe into which he has not yet penetrated should remain covered with lakes, marshes, and forests, which keep in store the juice of the earth, which the influence of the air, and the vivifying warmth of the sun would have expended long ere now in useless vegetation, and which the rains of heaven would have swept into the sea, had they not been thus protected by their situation or their cover. It is therefore the business of man to open up these mines of hoarded wealth, and to thank the Author of all Good, who has thus husbanded them for his use, and left them as a rightful heritage for those of after days.

The earth had not in the remote ages, as in our day, those great canals, those capacious voiders, always ready to drain off the rain waters (of which only part is absorbed by the thirsty ground), and the pure waters of the springs from the foot of the hills. The rivers did not then exist, or were only torrents, whose waters, confined by the gulleys and glens, are searching for a place to escape. Hence arise those numerous lakes in the interior of great continents, of which there are still remarkable relics in North America, which in process of time will disappear, and become champaign countries. The most remote from the sea, unable to contain its waters, finds an issue through some gorge of the hills, and pours over its superfluous waters into a lower basin, which, in its turn, discharges its contents into another, and the last of the chain delivers its waters by a river into the ocean. The communication was originally begun by a simple overflowing at the lowest part of the margin. This made a torrent, which quickly deepened its bed; and this circumstance increasing its velocity, as we have seen, would extend this deepening backward to the lake, and draw off more of its waters. The work would go on rapidly at first, while earth and small stones only resisted the labours of nature; but these being washed away, and the channel hollowed out to the firm rock on all sides, the operation must go on very slowly, till the immense cascade shall undermine what it cannot break off, and then a new discharge will commence, and a quantity of flat ground will emerge all round the lake. The torrent, in the mean time, makes its way down the country, and digs a canal, which may be called the first sketch of a river, which will deepen and widen its bed continually. The waters of several basins united, and running together in a great body, will (according to the principles we have established) have a much greater velocity, with the same slope, than those of the lakes in the interior parts of the continent; and the sum of them all united in the basin next the sea, after having broken through its natural mound, will make a prodigious torrent, which will dig for itself a bed so much the deeper as it has more slope and a greater body of waters.

The formation of the first valleys, by cutting open many springs, which were formerly concealed under ground, will add to the mass of running waters, and contribute to drain off the waters of these basins. In course of time many of them will disappear, and flat valleys among the mountains and hills are the traces of their former existence.

When nature thus traces out the courses of future rivers, it is to be expected that those streams will most deepen their channels which in their approach to the sea receive into their bed the greatest quantities of rain and spring waters, and that towards the middle of the continent they will deepen their channels less. In these last situations the natural slope of the fields causes the rain water, rills, and the little rivulets from the springs, to seek their way to the rivers. The ground can sink only by the flattening of the hills and high grounds; and this must proceed with extreme slowness, because it is only the gentle, though incessant work of the rains and springs. But the rivers, increasing in bulk and strength, and of necessity flowing over every thing, form to themselves capacious beds in a more yielding soil, and dig them even to the level of the ocean.

The beds of rivers by no means form themselves in one inclined plane. If we should suppose a canal AB (Fig. 13.) perfectly straight and horizontal at B, where it joins with the sea, this canal would really be an inclined channel of greater and greater slope as it is farther from B. This is evident; because gravity is directed towards the centre of the earth, and the angle CAB contained between the channel and the plumb-line at A is smaller than the similar angle CBD; and consequently the inclination to the horizon is greater in A than in D. Such a canal, therefore, would make the bed of a river; and some have thought that this was the real form of nature's work; but the supposition is a whim, and it is false. No river has a slope at all approaching to this. It would be 8 inches declivity in the mile next the ocean, 24 inches in the second mile, 40 inches in the third, and so on in the duplicate ratio (for the whole elevation) of the distances from the sea. Such a river would quickly tear up its bed in the mountains, (were there any grounds high enough to receive it), and, except its first cascade, would soon acquire a more gentle slope. But the fact is, and it is the result of the imprescriptible laws of nature, that the continued track of a river is a succession of inclined channels, whose slope diminishes by steps as the river approaches to the sea. It is not enough to say that this results from the natural slope of the countries through which it flows, which we observe to increase in declivity as we go to the interior parts of the continent. Were it otherwise, the equilibrium to which nature aims in all her operations would still produce the gradual diminution of the slope of rivers. Without it they could not be in a permanent train.

That we may more easily form a notion of the manner in which the permanent course of a river is established, let us suppose a stream or rivulet $s\,a$ (Fig. 14.) far up the country, make its way through a soil perfectly uniform to the sea, taking the course $s\,a\,b\,c\,d\,e\,f$; and receiving the *permanent additions* of the streams $g\,a$, $h\,b$, $i\,c$, $k\,d$, $l\,e$, and that its velocity and slope in all its parts are so suited to the tenacity of the soil and magnitude of its section, that neither do its waters during the annual freshes tear up its banks or deepen its bed, nor do they bring down from the high lands materials which they deposite in the channel in times of smaller velocity. Such a river may be said to be in a *permanent state*, to be in *conservation*, or to have *stability*. Let us call this state of a river its REGIMEN, denoting by the word the proper adjustment of the velocity of the stream to the tenacity of the channel. The velocity of its regimen must be the same throughout, because it is this which regulates its action on the bottom, which is the same from its head to the sea. That its bed may have stability, the mean velocity of the current must be constant, notwithstanding the inequality of discharge through its different sections by the brooks which it receives in its course, and notwithstanding the augmentation of its section as it approaches the sea.

On the other hand, it behoved this exact regimen to commence at the mouth of the river, by the working of the whole body of the river, in concert with the waters of the ocean, which always keep within the same limits, and make the ultimate level invariable. This working will begin to dig the bed, giving it as little breadth as possible: for this working consists chiefly in the efforts of falls and rapid streams, which arise of themselves in every channel which has too much slope. The bottom deepens, and the sides remain very steep, till they are undermined and crumble down; and being then diluted in the water, they are carried down the stream, and deposited where the ocean checks its speed. The banks crumble down anew, the valley or hollow forms; but the section, always confined to its bottom, cannot acquire a great breadth, and it retains a good deal of the form of the trapezium formerly mentioned. In this manner does the regimen begin to be established from *f* to *e*.

With respect to the next part *d e*, the discharge or produce is diminished by the want of the brook *l e*. It must take a similar form, but its area will be diminished, in order that its velocity may be the same; and its mean depth *d* being less than in the portion *e f* below, the slope must be greater. Without these conditions we could not have the uniform velocity, which the assumed permanency in an uniform soil necessarily supposes. Reasoning after the same manner for all the portions *c d*, *b c*, *a b*, *s a*, we see that the regimen will be successively established in them, and that the slope necessary for this purpose will be greater as we approach the river head. The vertical section or profile of the course of the river *s a b c d e f* will therefore resemble the line SABCDEF which is sketched below, having its different parts variously inclined to the horizontal line HF.

Such is the process of nature to be observed in every river on the surface of the globe. It long appeared a kind of puzzle to the theorists; and it was this observation of the increasing, or at least this continued velocity with smaller slope, as the rivers increased by the addition of their tributary streams, which caused Guglielmini to have recourse to his new principle, the energy of deep waters. We have now seen in what this energy consists. It is only a greater quantity of motion remaining in the middle of a great stream of water after a quantity has been retarded by the sides and bottom; and we see clearly, that since the addition of a new and perhaps an equal stream does not occupy a bed of double surface, the proportion of the retardations to the remaining motion must continually diminish as a river increases by the addition of new streams. If therefore the slope were not diminished, the regimen would be destroyed, and the river would dig up its channel. We have a full confirmation of this in the many works which have been executed on the Po, which runs with rapidity through a rich and yielding soil. About the year 1600, the waters of the Panaro, a very considerable river, were added to the Po Grande; and although it brings along with it in its freshes a vast quantity of sand and mud, it has greatly deepened the whole Tronco di Venezia from the confluence to the sea. This point was clearly ascertained by Manfredi about the year 1720, when the inhabitants of the valleys adjacent were alarmed by the project of bringing in the waters of the Rheno, which then ran through the Ferrarese. Their fears were overcome, and the Po Grande continues to deepen its channel every day with a prodigious advantage to the navigations; and there are several extensive marshes which now drain off by it, after having been for ages under water: and it is to be particularly remarked, that the Rheno is the foulest river in its freshes of any in that country. We insert this remark, because it may be of great practical utility, as pointing out a method of preserving and even improving the depth of rivers or drains in flat countries, which is not obvious, and rather appears improper: but it is strictly conformable to a true theory, and to the operations of nature, which never fails to adjust every thing so as to bring about an equilibrium. Whatever the declivity of the country may have been originally, the regimen begins to be settled at the mouths of the rivers, and the slopes are diminished in succession as we recede from the coast. The original slopes inland may have been much greater; but they will (when busy nature has completed her work) be left somewhat, and only so much greater, that the velocity may be the same notwithstanding the diminution of the section and mean depth.

Freshes will disturb this methodical progress relative only to the successive permanent additions; but their effects chiefly accelerate the deepening of the bed, and the diminution of the slope, by augmenting the velocity during their continuance. But when the regimen of the permanent additions is once established, the freshes tend chiefly to widen the bed, without greatly deepening it; for the aquatic plants, which have been growing and thriving during the peaceable state of the river, are now laid along, but not swept away, by the freshes, and protect the bottom from their attacks; and the stones and gravel, which must have been left bare in a course of years, working on the soil, will also collect in the bottom, and greatly augment its power of resistance; and even if the floods should have deepened the bottom some small matter, some mud will be deposited as the velocity of the freshes diminishes; and this will remain till the next flood.

We have supposed the soil uniform through the whole course: this seldom happens; therefore the circumstances which ensure permanency, or the regimen of a river, may be very different in its different parts, and in different rivers. We may say in general, that the farther that the regimen has advanced up the stream in any river, the more slowly will it convey its waters to the sea.

There are some general circumstances in the motion of rivers which it will be proper to take notice of just now, that they may not interrupt our more minute examination of their mechanism, and their explanations will then occur of themselves as corollaries of the propositions which we shall endeavour to demonstrate.

In a valley of small width the river always occupies the lowest part of it; and it is observed, that this is seldom in the middle of the valley, and is nearest to that side on which the slope from the higher grounds is steepest, and this without regard to the line of its course. The river generally adheres to the steepest hills, whether they advance into the plain or retire from it. This general feature may be observed over the whole globe. It is divided into copartments by great ranges of mountains; and it may be observed, that the great rivers hold their course not very far from them, and that their chief feeders come from the other side. In every copartment there is a swell of the low country at a distance from the bounding ridge of mountains; and on the summit of this swell the principal feeders of the great river have their sources.

The name *valley* is given with less propriety to these immense regions, and is more applicable to tracks of champaign land which the eye can take in at one view. Even here we may observe a resemblance. It is not always in the very lowest part of this valley that the river has its bed; although the waters of the river flow in a channel below its immediate banks, these banks are frequently higher than the grounds at the foot of the hills. This is very distinctly seen in Lower Egypt, by means of the canals which are carried backward from the Nile for accelerating its fertilizing inundations. When the calishes are opened to admit the waters, it is always observed that the districts most remote are the first covered, and it is several days before the immediately adjoining fields partake of the blessing. This is a consequence of that general operation of nature by which the valleys are formed. The river in its floods is loaded with mud, which it retains as long as it rolls rapidly along its limited bed, tumbling its waters over and over, and taking up in every spot as much as it deposites: but as soon as it overflows its banks, the very enlargement of its section diminishes the velocity of the water; and it may be observed still running in the track of its bed with great velocity, while the waters on each side are stagnant at a very small distance: therefore the water, on getting over the banks, must deposite the heaviest, the firmest, and even the greatest part of its burden, and must become gradually clearer as it approaches the hills. Thus a gentle slope is given as the valley in a direction which is the reverse of what one would expect. It is, however, almost always the case in wide valleys, especially if the great river comes through a soft country. The banks of the brooks and ditches are observed to be deeper as they approach the river, and the merely superficial drains run backwards from it.

We have already observed, that the enlargement of the bed of a river, in its approach to the sea, is not in proportion to the increase of its waters. This would be the case even if the velocity continued the same: and therefore, since the velocity even increases, in consequence of the greater energy of a large body of water, which we now understand distinctly, a still smaller bed is sufficient for conveying all the water to the sea.

This general law is broken, however, in the immediate neighbourhood of the sea; because in this situation the velocity of the water is checked by the passing flood-tides of the ocean. As the whole waters must still be discharged, they require a larger bed, and the enlargement will be chiefly in width. The sand and mud are deposited when the motion is retarded. The depth of the mouth of the channel is therefore diminished. It must therefore become wider. If this be done on a coast exposed to the force of a regular tide, which carries the waters of the ocean across the mouth of the river, this regular enlargement of the mouth will be the only consequence, and it will generally widen till it washes the foot of the adjoining hills; but if there be no tide in the sea, or a tide which does not set across the mouth of the river, the sands must be deposited at the sides of the opening, and become additions to the shore, lengthening the mouth of the channel. In this sheltered situation, every trivial circumstance will cause the river to work more on particular parts of the bottom, and deepen the channel there. This keeps the mud suspended in such parts of the channel, and it is not deposited till the stream has shot farther out into the sea. It is deposited on the sides of those deeper parts of the channel, and increases the velocity in them, and thus still further protracts the deposition. Rivers so situated will not only lengthen their channels, but will divide them, and produce islands at their mouths. A bush, a tree torn up by the roots by a mountain torrent, and floated down the stream, will thus inevitably produce an island; and rivers in which this is common will be continually shifting their mouths. The Mississippi is a most remarkable instance of this. It has a long course through a rich soil, and disembogues itself into the bay of Mexico, in a place where there is no *passing tide*, as may be seen by comparing the hours of high water in different places. No river that we know carries down its stream such numbers of rooted-up trees: they frequently interrupt the navigation, and render it always dangerous in the night-time. This river is so beset with flats and shifting sands at its mouth, that the most experienced pilots are puzzled; and it has protruded its channel above 50 miles in the short period that we have known it. The discharge of the Danube is very similar: so is that of the Nile; for it is discharged into a still corner of the Mediterranean. It may now be said to have acquired considerable permanency; but much of this is owing to human industry, which strips it as much as possible of its subsideable matter. The Ganges too is in a situation pretty similar, and exhibits similar phenomena. The Maragnon might be noticed as an exception; but it is not an exception. It has flowed very far in a level bed, and its waters come pretty clear to Para; but besides, there is a strong tranverse tide, or rather current, at its mouth, setting to the south-east both during flood and ebb. The mouth of the Po is perhaps the most remarkable of any on the surface of this globe, and exhibits appearances extremely singular. Its discharge is into a sequestered corner of the Adriatic. Though there be a more remarkable tide in this gulf than in any part of the Mediterranean, it is still but trifling, and it either sets directly in upon the mouth of the river, or retires straight away from it. The river has many mouths, and they shift prodigiously. There has been a general increase of the land very remarkable. The marshes where Venice now stands were, in the Augustan age, everywhere penetrable by the fishing-boats, and in the 5th century could only bear a few miserable huts; now they are covered with crowds of stately buildings. Ravenna, situated on the southernmost mouth of the Po, was, in the Augustan age, at the extremity of a swamp, and the road to it was along the top of an artificial mound, made by Augustus at immense expense. It was, however, a fine city, containing extensive docks, arsenals, and other massy buildings, being the great military port of the empire, where Augustus laid up his great ships of war. In the Gothic times it became almost the capital of the Western empire, and was the seat of government and of luxury. It must, therefore, be supposed to have every accommodation of opulence, and we cannot doubt of its having paved streets, wharfs, &c.; so that its wealthy inhabitants were at least walking dry-footed from house to house. But now it is an Italian mile from the sea, and surrounded with vineyards and cultivated fields, and is accessible in every direction. All this must have been formed by depositions from the Po, flowing through Lombardy loaded with the spoils of the Alps, which were here arrested by the reeds and bulrushes of the marsh. These things are in common course; but when wells are dug, we come to the pavements of the ancient city, and these pavements are all on one exact level, and they *are eight feet below the surface of the sea at low water*. This cannot be ascribed to the subsiding of the ancient city. This would be irregular, and greatest among the heavy buildings. The tomb of Theodoric remains, and the pavement round it is on a level with all the others. The lower story is always full of water; so is the lower story of the cathedral to the depth of three feet. The ornaments of both these buildings leave no room to doubt that they were formerly dry; and such a building as the cathedral could not sink without crumbling into pieces.

It is by no means easy to account for all this. The depositions of the Po and other rivers must raise the ground; and yet the rivers must still flow over all. We must conclude that the surface of the Adriatic is by no means level, and that it slopes like a river from the Lagoon of Venice to the eastward. In all probability it even slopes considerably outwards from the shore.

The last general observations which we shall make in this place is, that the surface of a river is not flat, considered athwart the stream, but convex: this is owing to its motion. Suppose a canal of stagnant water; its surface would be a perfect level. But suppose it possible by any means to give the middle waters a motion in the direction of its length, they must drag along with them the waters immediately contiguous. These will move less swiftly, and will in like manner drag the waters without them; and thus the water at the sides being abstracted, the depth must be less, and the general surface must be convex across. The fact in a running stream is similar to this; the side waters are withheld by the sides, and every filament is moving more slowly than the one next it towards the middle of the river, but faster than the adjoining filament on the land side. This alone must produce a convexity of surface. But besides this, it is demonstrable that the pressure of a running stream is diminished by its motion, and the diminution is proportional to the height which would produce the velocity with which it is gliding past the adjoining filament. This convexity must in all cases be very small. Few rivers have the velocity nearly equal to eight feet *per* second, and this requires a height of one foot only.

SECT. 4.—*Of the Windings of Rivers.*

RIVERS are seldom straight in their course. Formed by the hand of nature, they are accommodated to every change of circumstance. They wind around what they cannot get over, and work their way to either side according as the resistance of the opposite bank makes a straight course more difficult; and this seemingly fortuitous rambling distributes them more uniformly over the surface of a country, and makes them everywhere more at hand, to receive the numberless rills and rivulets which collect the waters of our springs and the superfluities of our showers, and to comfort our habitations with the many advantages which cultivation and society can derive from their presence. In their feeble beginnings the smallest inequality of slope or consistency is enough to turn them aside and make them ramble through every field, giving drink to our herds and fertility to our soil. The more we follow nature into the minutiæ of her operations, the more must we admire the inexhaustible fertility of her resources, and the simplicity of the means by which she produces the most important and beneficial effects. By thus twisting the course of our rivers into 10,000 shapes, she keeps them long amidst our fields, and thus compensates for the declivity of the surface, which otherwise would tumble them with great rapidity into the ocean, loaded with the best and richest of our soil. Without this, the showers of heaven would have little influence in supplying the waste of incessant evaporation. But as things are, the rains are kept slowly trickling along the sloping sides of our hills and steeps, winding round every clod, nay, every plant, which lengthens their course, diminishes their slope, checks their speed, and thus prevents them from quickly brushing off from every part of the surface the lightest and best of the soil. The flattest of our holm lands would be too steep, and the rivers would shoot along through our finest meadows, hurrying every thing away with them, and would be unfit for the purposes of inland conveyance, if the inequalities of soil did not make them change this headlong course for the more beautiful meanders which we observe in the course of the small rivers winding through our meadows. Those rivers are in general the straightest in their course which are the most rapid, and which roll along the greatest bodies of water; such are the Rhone, the Po, the Danube. The smaller rivers continue more devious in their progress, till they approach the sea, and have gathered strength from all their tributary streams.

Every thing aims at an equilibrium, and this directs even the ramblings of rivers. It is of importance to understand the relation between the force of a river and the resistance which the soil opposes to those deviations from a rectilineal course; for it may frequently happen that the general procedure of nature may be inconsistent with our local purposes. Man was set down on this globe, and the task of cultivating it was given him by nature, and his chief enjoyment seems to be to struggle with the elements. He must not find things to his mind, but he must mould them to his own fancy. Yet even this seeming anomaly is one of nature's most beneficent laws; and his exertions must still be made in conformity with the general train of the operations of mechanical nature: and when we have any work to undertake relative to the course of rivers, we must be careful not to thwart their general rules, otherwise we shall be sooner or later punished for their infraction. Things will be brought back to their former state, if our operations are inconsistent with that equilibrium which is constantly aimed at, or some new state of things which is equivalent will be soon induced. If a well-regulated river has been improperly deepened in some place, to answer some particular purpose of our own, or if its breadth has been improperly augmented, we shall soon see a deposition of mud or sand choke up our fancied improvements; because, as we have enlarged the section without increasing the slope or the supply, the velocity must diminish, and floating matters must be deposited.

It is true, we frequently see permanent channels where the forms are extremely different from that which the waters would dig for themselves in an uniform soil, and which approaches a good deal to the trapezium described formerly. We see a greater breadth frequently compensate for a want of depth; but all such deviations are a sort of constraint, or rather are indications of inequality of soil. Such irregular forms are the works of nature; and if they are permanent, the equilibrium is obtained. Commonly the bottom is harder than the sides, consisting of the coarsest of the sand and of gravel; and therefore the necessary section can be obtained only by increasing the width. We are accustomed to attend chiefly to the appearances which prognosticate mischief, and we interpret the appearances of a permanent bed in the same way, and frequently form very false judgments. When we see one bank low and flat, and the other high and abrupt, we suppose that the waters are passing along the first in peace, and with a gentle stream, but that they are rapid on the other side, and are tearing away the bank; but it is just the contrary. The bed being permanent, things are in equilibrio, and each bank is of a form just competent to that equilibrium. If the soil on both sides be uniform, the stream is most rapid on that side where the bank is low and flat, for in no other form would it withstand the action of the stream; and it has been worn away till its flatness compensates for the greater force of the stream. The stream on the other side must be more gentle, otherwise the bank could not remain abrupt. In short, in a state of permanency, the velocity of the stream and form of the bank are just suited to each other. It is quite otherwise before the river has acquired its proper regimen.

A careful consideration therefore of the general features of rivers which have settled their regimen, is of use for informing us concerning their internal motions, and directing us to the most effectual methods of regulating their course.

We have already said that perpendicular brims are inconsistent with stability. A semicircular section is the form which would produce the quickest train of a river whose expense and slope are given; but the banks at B and D (Fig. 15.) would crumble in, and lie at the bottom, where their horizontal surface would secure them from farther change. The bed will acquire the form G *c* F, of equal section, but greater width, and with brims less shelving. The proportion of the velocities at A and *c* may be the same with that of the velocities at A and C; but the velocity at G and F will be less than it was formerly at B, C, or D; and the velocity in any intermediate point E, being somewhat between those at F and *c*, must be less than it was in any intermediate point of the semicircular bed. The velocities will therefore decrease along the border from *c* towards G and F, and the steepness of the border will augment at the same time, till, in every point of the new border G *c* F, these two circumstances will be so adjusted that the necessary equilibrium is established.

The same thing must happen in our trapezium. The slope of the brims may be exact, and will be retained; it will, however, be too great anywhere below, where the velocity is greater, and the sides will be worn away till the banks are undermined and crumble down, and the river will maintain its section by increasing its width. In short, no border made up of straight lines is consistent with that gradation of velocity which will take place whenever we depart from a semicircular form. And we accordingly see, that in all natural channels the section has a curvilineal border, with the slope increasing gradually from the bottom to the brim.

These observations will enable us to understand how nature operates when the inequality of surface or of tenacity obliges the current to change its direction, and the river forms an elbow.

Supposing always that the discharge continues the same, and that the mean velocity is either preserved or restored, the following conditions are necessary for a permanent regimen:

1. The depth of water must be greater in the elbow than any where else.

2. The main stream, after having struck the concave bank, must be reflected in an equal angle, and must then be in the direction of the next reach of the river.

3. The angle of incidence must be proportioned to the tenacity of the soil.

4. There must be in the elbow an increase of slope, or of head of water, capable of overcoming the resistance occasioned by the elbow.

The reasonableness, at least, of these conditions will appear from the following considerations:

1. It is certain that force is expended in producing this change of direction in a channel which, by supposition, diminishes the current. The diminution arising from any cause which can be compared with friction must be greater when the stream is directed against one of the banks. It may be very difficult to state the proportion, and it would occupy too much of our time to attempt it; but it is sufficient that we be convinced that the retardation is greater in this case. We see no cause to increase the mean velocity in the elbow, and we must therefore conclude that it is diminished. But we are supposing that the discharge continues the same; the section must therefore augment, or the channel increase its transverse dimensions. The question is, in what manner it does this, and what

change of form does it affect, and what form is competent to the final equilibrium and the consequent permanency of the bed? Here there is much room for conjecture. M. Buat reasons as follows: If we suppose that the points B and C (Fig. 16.) continue on a level, and that the points H and I at the beginning of the next reach are also on a level, it is an inevitable consequence that the slope along CMI must be greater than along BEH, because the depression of H below B is equal to that of I below C, and BEH is longer than CMI. Therefore the velocity along the convex bank CMI must be greater than along BEH. There may even be a stagnation and an eddy in the contrary direction along the concave bank. Therefore, if the form of the section were the same as up the stream, the sides could not stand on the convex bank. When therefore the section has attained a permanent form, and the banks are again in equilibrio with the action of the current; the convex bank must be much flatter than the concave. If the water is ready still on the concave bank, that bank will be absolutely perpendicular; nay, may overhang.—Accordingly, this state of things is matter of daily observation, and justifies our reasoning, and entitles us to say, that this is the nature of the internal motion of the filaments which we cannot distinctly observe. The water moves most rapidly along the convex bank, and the thread of the stream is nearest to this side. Reasoning in this way, the section, which we may suppose to have been originally of the form M *b* *a* E, (Fig. 17.) assumes the shape MBAE.

2. Without presuming to know the mechanism of the internal motions of fluids, we know that superficial waves are reflected precisely as if they were elastic bodies, making the angles of incidence and reflection equal. In as far therefore as the superficial wave is concerned in the operation, M. Buat's second position is just. The permanency of the next reach requires that its axis shall be in the direction of the line EP, which makes the angle GEP =

FEN. If the next reach has the direction EQ, MR, the wave reflected in the line ES will work on the bank at S, and will be reflected in the line ST, and work again on the opposite bank at T. We know that the effect of the superficial motion is great, and that it is the principal agent in destroying the banks of canals. So far therefore M. Buat is right. We cannot say with any precision or confidence how the actions of the under filaments are modified; but we know no reason for not extending to the under filaments what appears so probable with respect to the surface water.

3. The third position is no less evident. We do not know the mode of action of the water on the bank; but our general notions on this subject, confirmed by common experience, tell us that the more obliquely a stream of water beats on any bank, the less it tends to undermine it or wash it away. A stiff and cohesive soil therefore will suffer no more from being almost perpendicularly buffeted by a stream than a friable sand would suffer from water gliding along its face. M. Buat thinks, from experience, that a clay bank is not sensibly affected till the angle FEB is about 36 degrees.

4. Since there are causes of retardation, and we still suppose that the discharge is kept up, and that the mean velocity, which had been diminished by the enlargement of the section, is again restored, we must grant that there is provided, in the mechanism of these motions, an accelerating force adequate to this effect. There can be no accelerating force in an open stream but the superficial slope. In the present case it is undoubtedly so; because by the deepening of the bottom where there is an elbow in the stream, we have of necessity a counter slope. Now, all this head of water, which must produce the augmentation of velocity in that part of the stream which ranges round the convex bank, will arise from the check which the water gets from the concave bank. This occasions a gorge or swell

up the stream, enlarges a little the section at BVC; and this, by the principal of uniform motion, will augment all the velocities, deepen the channel, and put every thing again into its train as soon as the water gets into the next reach. The water at the bottom of this basin has very little motion, but it defends the bottom by this very circumstance.

Such are the notions which M. de Buat entertains of this part of the mechanism of running waters. We cannot say that they are very satisfactory, and they are very opposite to the opinions commonly entertained on the subject. Most persons think that the motion is most rapid and turbulent on the side of the concave bank, and that it is owing to this that the bank is worn away till it become perpendicular, and that the opposite bank is flat, because it has not been gnawed away in this manner. With respect to this general view of the matter, these persons may be in the right; and when a stream is turned into a crooked and yielding channel for the first time, this is its manner of action. But M. Buat's aim is to investigate the circumstances which obtain in the case of a regimen; and in this view he is undoubtedly right as to the facts, though his mode of accounting for these facts may be erroneous. And as this is the only useful view to be taken of the subject, it ought chiefly to be attended to in all our attempts to procure stability to the bed of a river, without the expensive helps of masonry, &c. If we attempt to secure permanency by deepening on the inside of the elbow, our bank will undoubtedly crumble down, diminish the passage, and occasion a more violent action on the hollow bank. The most effectual mean of security is to enlarge the section; and if we do this on the inside bank, we must do it by widening the stream very much, that we may give a very sloping bank. Our attention is commonly drawn to it when the hollow bank is giving way, and with a view to stop the ravages of the stream. Things are not now in a state of permanency, but nature is working in her own way to bring it about. This may not suit *our* purpose, and we must thwart her. The phenomena which we then observe are frequently very unlike to those described in the preceding paragraphs. We see a violent tumbling motion in the stream towards the hollow bank. We see an evident accumulation of water on that side, and the point B is frequently higher than C. This regorging of the water extends to some distance, and is of itself a cause of greater velocity, and contributes, like a head of stagnant water, to force the stream through the bend, and to deepen the bottom. This is clearly the case when the velocity is excessive, and the hollow bank able to abide the shock. In this situation the water thus heaped up escapes where it best can; and as the water, obstructed by an obstacle put in its way, escapes by the sides, and there has its velocity increased, so here the water gorged up against the hollow bank swells over towards the opposite side, and passes round the convex bank with an increased velocity. It depends much on the adjustment between the velocity and consequent accumulation, and the breadth of the stream and the angle of the elbow, whether this augmentation of velocity shall reach the convex bank; and we sometimes see the motion very languid in that place, and even depositions of mud and sand are made there. The whole phenomena are too complicated to be accurately described in general terms, even in the case of perfect regimen: for this regimen is relative to the consistence of the channel; and when this is very great, the motions may be most violent in every quarter. But the preceding observations are of importance, because they relate to ordinary cases and to ordinary channels.

It is evident, from M. Buat's second position, that the proper form of an elbow depends on the breadth of the stream as well as on the radius of curvature, and that every angle of elbow will require a certain proportion between the width of the river and the radius of the sweep. M. Buat gives rules and formulæ for all these purposes, and shows that in one sweep there may be more than one reflection or rebound. It is needless to enlarge on this matter of mere geometrical discussion. It is with the view of enabling the engineer to trace the windings of a river in such a manner that there shall be no rebounds which shall direct the stream against the sides, but preserve it always in the axis of every reach. This is of consequence, even when the bends of the river are to be secured by masonry or piling; for we have seen the necessity of increasing the section, and the tendency which the waters have to deepen the channel on that side where the rebound is made. This tends to undermine our defences, and obliges us to give them deeper and more solid foundations in such places. But any person accustomed to the use of the scale and compasses will form to himself rules of practice equally sure and more expeditious than M. de Buat's formulæ.

We proceed, therefore, to what is more to our purpose, the consideration of the resistance caused by an elbow, and the methods of providing a force capable of overcoming it. We have already taken notice of the salutary consequences arising from the rambling course of rivers, inasmuch as it more effectually spreads them over the face of a country. It is no less beneficial by diminishing their velocity. This it does both by lengthening their course, which diminishes the declivity, and by the very resistance which they meet with at every bend. We derive the chief advantages from our rivers, when they no longer shoot their way from precipice to precipice, loaded with mud and sand, but peaceably roll along their clear waters, purified during their gentler course, and offer themselves for all the purposes of pasturage, agriculture, and navigation. The more a river winds its way round the foot of the hills, the more is the resistance of its bed multiplied; the more obstacles it meets with in its way from its source to the sea, the more moderate is its velocity; and instead of tearing up the very bowels of the earth, and digging for itself a deep trough; along which it sweeps rocks and rooted-up trees, it flows with majestic pace even with the surface of our cultivated grounds, which it embellishes and fertilizes.

We may with safety proceed on the supposition, that the force necessary for overcoming the resistance arising from a rebound is as the square of the velocity; and it is reasonable to suppose it proportional to the square of the sine of the angle of incidence, and this for the reasons given for adopting this measure of the general RESISTANCE OF FLUIDS. It cannot, however, claim a greater confidence here than in that application; and it has been shown in that article with what uncertainty and limitations it must be received. We leave it to our readers to adopt either this or the simple ratio of the sines, and shall abide by the duplicate ratio with M. Buat, because it appears by his experiments that this law is very exactly observed in tubes in inclinations not exceeding 40°; whereas it is in these small angles that the application to the general resistance of fluids is most in fault. But the correction is very simple, if this value shall be found erroneous. There can be little doubt that the force necessary for overcoming the resistance will increase as the number of rebounds.—Therefore we may express the resistance, in general, by the formula $r = \frac{V^2 s^2 n}{m}$; where r is the resistance, V the mean velocity of the stream, s the sine of the angle of incidence, n the number of equal rebounds (that is, having equal angles of incidence), and m is a number to be determined by experiment. M. de Buat made many experiments on the resistance occasioned by the bendings of pipes, none of which differed from the result of the above formula above one part in twelve; and he concludes, that the resistance to one bend may be estimated at $\frac{V^2 s^2}{3000}$. The experiment was in this form: A pipe of 1 inch diameter, and 10 feet long, was formed with 10 rebounds of 36° each. A head of water was applied to it, which gave the water a velocity of six feet *per second*. Another pipe of the same diameter and length, but without any bendings, was subjected to a pressure of a head of water, which was increased till the velocity of efflux was also six feet *per* second. The additional head of water was $5\frac{1}{10}$ inches. Another of the same diameter and length, having one bend of 24° 34′, and running 85 inches *per* second, was compared with a straight pipe having the same velocity, and the difference of the heads of water was $\frac{37}{100}$ of an inch. A computation from these two experiments will give the above result, or, in English measure, $r = \frac{V^2 s^2}{3200}$ very nearly. It is probable that this measure of the resistance is too great; for the pipe was of uniform diameter even in the bends; whereas in a river properly formed, where the regimen is exact, the capacity of the section of the bend is increased.

The application of this theory to inclined tubes and to open streams is very obvious, and very legitimate and safe. Let AB (Fig. 18.) be the whole height of the reservoir ABIK, and BC the horizontal length of a pipe, containing any number of rebounds, equal or unequal, but all regular; that is, constructed according to the conditions formerly mentioned. The whole head of water should be conceived as performing, or as divided into portions which perform, three different offices.—One portion $AD = \frac{V^2}{505}$, impels the water into the entry of the pipe with the velocity with which it really moves in it; another portion EB is in equilibrio with the resistances arising from the mere length of the pipe expanded into a straight line; and the third portion DE serves to overcome the resistance of the bends. If, therefore, we draw the horizontal line BC, and, taking the pipe BC out of its place, put it in the position DH, with its mouth C in H, so that DH is equal to BC, the water will have the same velocity in it that it had before. *N. B.*—For greater simplicity of argument, we may suppose that when the pipe was inserted at B, its bends all lay in a horizontal plane, and that when it is inserted at D, the plane in which all its bends lie, slopes only in the direction DH, and is perpendicular to the plane of the figure. We repeat it, the water will have the same velocity in the pipes BC and DH, and the resistances will be overcome. If we now prolong the pipe DH towards L to any distance, repeating continually the same bendings in a series of lengths, each equal to DH, the motion will be continued with the velocity corresponding to the pressure of the column AD; because the declivity of the pipe is augmented in each length equal to DH, by a quantity precisely sufficient for overcoming all the resistances in that length; and the true slope in these cases is BE + ED, divided by the expanded length of the pipe BC or DH.

The analogy which we were enabled to establish between the uniform motion or the train of pipes and of open streams, entitles us now to say, that when a river has bendings, which are regularly repeated at equal intervals, its slope is compounded of the slope which is necessary for overcoming the resistance of a straight channel of its whole expanded length, agreeably to the formula for uniform motion, and of the slope which is necessary for overcoming the resistance arising from its bendings alone.

Thus, let there be a river which, in the expanded course of 6000 fathoms, has 10 elbows, each of which has 30° of rebound; and let its mean velocity be 20 inches in a second. If we would learn its whole slope in this 6000 fathoms, we must first find (by the formula of uniform motion) the slope *s* which will produce the velocity of 20 inches in a straight river of this length, section, and mean depth. Suppose this to be $\frac{1}{21000}$, or 20 inches in this whole length. We must then find (by the formula $\frac{V^2 \text{Sin}^2}{3200}$) the slope necessary for overcoming the resistance of 10 rebounds of 30° each. This we shall find to be $6\frac{2}{3}$ inches in the 6000 fathoms. Therefore the river must have a slope of $26\frac{2}{3}$ inches in 6000 fathoms, or $\frac{1}{16200}$; and this slope will produce the same velocity which 20 inches, or — $\frac{1}{21600}$, would do in a straight running river of the same length.

PART II.—PRACTICAL INFERENCES.

HAVING thus established a theory of a most important part of hydraulics, which may be confided in as a just representation of nature's procedure, we shall apply it to the examination of the chief results of every thing which art has contrived for limiting the operations of nature, or modifying them so as to suit our particular views. Trusting to the detail which we have given of the connecting principles, and the chief circumstances which co-operate in producing the ostensible effect; and supposing that such of our readers as are interested in this subject will not think it too much trouble to make the applications in the same detail; we shall content ourselves with merely pointing out the steps of the process, and showing their foundation in the theory itself: and frequently, in place of the direct analysis which the theory enables us to employ for the solution of the problems, we shall recommend a process of approximation by trial and correction, sufficiently accurate, and more within the reach of practical engineers. We are naturally led to consider in order the following articles

1. The effects of permanent additions of every kind to the waters of a river, and the most effectual methods of preventing or removing inundations.

2. The effects of weirs, bars, sluices, and keeps of every kind, for raising the surface of a river; and the similar

effects of bridges, piers, and every thing which contracts the section of the stream.

3. The nature of canals; how they differ from rivers in respect of origin, discharge, and regimen, and what conditions are necessary for their most perfect construction.

4. Canals for draining land, and drafts or canals of derivation from the main stream. The principles of their construction, so that they may suit their intended purposes, and the change which they produce on the main stream, both above and below the point of derivation.

Of the Effects of permanent Additions to the Waters of a River.

FROM what has been said already, it appears that to every kind of soil or bed there corresponds a certain velocity of current, too small to hurt it by digging it up, and too great to allow the deposition of the materials which it is carrying along. Supposing this known for any particular situation, and the quantity of water which the channel must of necessity discharge, we may wish to learn the smallest slope which must be given to this stream, that the waters may run with the required velocity. This suggests

PROB. I. Given the discharge D of a river, and V its velocity of regimen: required the smallest slope s, and the dimensions of its bed?

Since the slope must be the smallest possible, the bed must have the form which will give the greatest mean depth d, and should therefore be the trapezium formerly described; and its area and perimeter are the same with those of a rectangle whose breadth is twice its height h. These circumstances give us the equation $\frac{D}{V} = 2h^2$. For the area of the section is twice the square of the height, and the discharge is the product of this area and the velocity. Therefore $\sqrt{\frac{D}{2V}} = h$ and $\sqrt{\frac{2D}{V}} =$ the breadth b.

The formula of uniform motion gives $\sqrt{s} - L\sqrt{s+1,6}$ $= \frac{297(\sqrt{d}-0,1}{V+0,3(\sqrt{d}-0,1)}$. Instead of $\sqrt{d}-0,1$, put its equal $\sqrt{\frac{h}{2}} - 0,1$, and every thing being known in the second member of this equation, we easily get the value of s by a few trials after the following manner. Suppose that the second member is equal to any number, such as 9. First suppose that $\sqrt{s}$ is $= 9$. Then the hyperbolic logarithm of $9+1,6$ or of $10,6$ is $2,36$. Therefore we have $\sqrt{s} - L\sqrt{s+1,6} = 9 - 2,36, = 6,64$; whereas it should have been $= 9$. Therefore say $6,64 : 9 = 9 : 12,2$ nearly. Now suppose that $\sqrt{s}$ is $= 12,2$. Then $L\overline{12,2+1,6} = L13,8$, $= 2,625$ nearly, and $12,2 - 2,625$ is $9,575$, whereas it should be 9. Now we find that changing the value of $\sqrt{s}$ from 9 to 12,2 has changed the answer from 6,64 to 9,575, or a change of 3,2 in our assumption has made a change of 2,935 in the answer, and has left an error of 0,575. Therefore say $2,935 : 0,575 = 3,2 : 0,628$. Then, taking 0,628 from 12,2, we have (for our next assumption or value of $\sqrt{s}$) 11,572. Now $11,572 + 1,6 = 13,172$, and $L\,13,172$ is 2,58 nearly. Now try this last value $11,572 - 2,58$ is 9,008, sufficiently exact. This may serve as a specimen of the trials by which we may avoid an intricate analysis.

Prob. II. Given the discharge D, the slope s, and the velocity V, of permanent regimen, to find the dimensions of the bed.

Let x be the width, and y the depth of the channel, and S the area of the section. This must be $= \frac{D}{V}$, which is therefore $= xy$ The denominator s being given, we may make $\sqrt{s} - L\sqrt{s+1{,}6} = \sqrt{B}$, and the formula of mean velocity will give $V = \frac{297(\sqrt{d}-0{,}1)}{\sqrt{B}} - 0{,}3(\sqrt{d}-0{,}1)$, which we may express thus: $V = (\sqrt{d}-0{,}1)\left(\frac{97}{\sqrt{B}}-0{,}3\right)$, which gives $\frac{V}{\frac{297}{\sqrt{B}}-0{,}3} = \sqrt{d} - 0{,}1)$; and finally,

$$\frac{V}{\frac{297}{\sqrt{B.}}-0{,}3} + 0{,}1 = \sqrt{d}.$$

Having thus obtained what we called the mean depth, we may suppose the section rectangular. This gives $d = \frac{xy}{x+2y}$. Thus we have two equations, $S = xy$ and $d = \frac{xy}{x+2y}$.

From which we obtain $x = \sqrt{\left(\frac{S}{2d}\right)^2 - 2S} + \frac{S}{2d}$. And having the breadth x and area S, we have $y = \frac{S}{x}$. And then we may change this for the trapezium often mentioned.

These are the chief problems on this part of the subject, and they enable us to adjust the slope and channel of a river which receives any number of successive permanent additions by the influx of other streams. This last informs us of the rise which a new supply will produce, because the additional supply will require additional dimensions of the channel; and as this is not supposed to increase in breadth, the addition will be in depth. The question may be proposed in the following problem:

Prob. III. Given the slope s, the depth and the base of a rectangular bed (or a trapezium), and consequently the discharge D, to find how much the section will rise, if the discharge be augmented by a given quantity.

Let h be the height after the augmentation, and w the width for the rectangular bed. We have in any uniform current $\sqrt{d} = \dfrac{V}{\dfrac{297}{\sqrt{B}} - 0{,}3}$. Raising this to a square, and putting for d and V their values $\dfrac{w\,h}{w+2\,h}$ and $\dfrac{D}{wh}$ and making $\dfrac{297}{\sqrt{B}} - 0{,}3 = K$, the equation becomes $\dfrac{w\,h}{w+2h} = \left(\dfrac{D}{w\,h\,K} + 0{,}1\right)^2$. Raising the second member to a square, and reducing, we obtain a cubic equation, to be solved in the usual manner.

But the solution would be extremely complicated. We may obtain a very expeditious and exact approximation from this consideration, that a small change in one of the dimensions of the section will produce a much greater change in the section and the discharge than in the mean depth d. Having therefore augmented the unknown dimension, which is here the height, make use of this to form a new mean depth, and then the new equation $\sqrt{d} = \dfrac{D}{wh\left(\dfrac{297}{\sqrt{B}} - 0{,}3\right)} + 0{,}1$ will give us another value of h, which will rarely exceed the truth by $\frac{1}{10}$. This serves (by the same process) for finding another, which will commonly be sufficiently exact. We shall illustrate this by an example.

Let there be a river whose channel is a rectangle 150 feet wide and six feet deep, and which discharges 1500 cubic feet of water per second, having a velocity of 20 inches, and slope of $\frac{1}{12800}$, or about $\frac{7}{12}$ of an inch in 100 fathoms. How much will it rise if it receives an addition which triples its discharge? and what will be its velocity?

If the velocity remained the same, its depth would be tripled; but we know by the general formula that its velocity will be greatly increased, and therefore its depth will not be tripled. Suppose it to be doubled, and to become 12 feet. This will give $d = 10,34483$, or 124,138 inches; then the equation $\sqrt{d} - 0,1 = \frac{D}{wh\left(\frac{297}{\sqrt{B}} - 0,3\right)}$ or $h = \frac{D}{w(\sqrt{d} - 0,1)\left(\frac{297}{\sqrt{B}} - 0,3\right)}$, and in which we have $\sqrt{B} = 107,8$, $D = 4500$; $\sqrt{d} - 0,1 = 11,0417$, will give $h = 13,276$; whereas it should have been 12. This shows that our calculated value of d was too small. Let us therefore increase the depth by 0,9, or make it 12,9, and repeat the calculation. This will give us $\sqrt{d} - 0,1 = 11,3087$, and $h = 12,867$, instead of 13,276. Therefore augmenting our data 0,9 changes our answer 0,409. If we suppose these small changes to retain their proportions, we may conclude that if 12 be augmented by the quantity $x \times 0,9$, the quantity 13,276 will diminish by the quantity $x \times 0,409$. Therefore, that the estimated value of h may agree with the one which results from the calculation, we must have $12 + x \times 0,9 = 13,276 - x \times 0,409$. This will give $x = \frac{1,276}{1,309}$, $= 0,9748$, and $x \times 0,9 = 0,8773$; and $h = 12,8773$. If we repeat the calculation with this value of h, we shall find no change.

This value of h gives $d = 131,8836$ inches. If we now compute the new velocity by dividing the new discharge 4500 by the new area $150 \times 12,8773$, we shall find it to be 27,95 inches, in place of 20, the former velocity.

We might have made a pretty exact first assumption, by recollecting what was formerly observed, that when the breadth is very great in proportion to the depth, the mean depth differs insensibly from the real depth, or rather follows nearly the same proportions, and that the velocities are proportional to the square roots of the depths. Call the the first discharge d, the height h, and velocity v, and let D, H, and V, express these things in their augmented state. We have $v = \frac{d}{wh}$ and $V = \frac{D}{wH}$, and $v : V = \frac{d}{h} : \frac{D}{H}$, and $v^2 : V^2 = \frac{d^2}{h^2} : \frac{D^2}{H^2}$. But by this remark $v^2 : V^2 = h : H$. Therefore $h : H = \frac{d^2}{h^2} : \frac{D^2}{H^2}$ and $\frac{hD^2}{H^2} = \frac{Hd^2}{h^2}$, and $h^3 D^2 = H^3 d^2$, and $d^2 : D^2 = h^3 : H^3$ (a useful theorem) and $H^3 = \frac{h^3 D^2}{d^2}$, and $H = \sqrt[3]{\frac{h^3 D^2}{d^2}} = 12{,}48$

Or we might have made the same assumption by the remark also formerly made on this case, that the squares of the discharges are nearly as the cubes of the height, or $1500^2 : 4500^2 = 6^3 : 22{,}48^3$.

And in making these first guesses we shall do it more exactly, by recollecting that a certain variation of the mean depth d requires a greater variation of the height, and the increment will be to the height nearly as half the height to the width, as may easily be seen. Therefore, if we add to 12,48 its $\frac{6{,}24}{150}$th part, or its 24th part, viz. 0,52, we have 13 for our first assumption, exceeding the truth only an inch and a half. We mention these circumstances, that those who are disposed to apply these doctrines to the solution of practical cases may be at no loss when one occurs of which the regular solution requires an intricate analysis.

It is evident that the inverse of the foregoing problems will show the effects of enlarging the section of a river, that is, will show how much its surface will be sunk by any proposed enlargement of its bed. It is therefore needless to propose such problems in this place. Common sense directs us to make these enlargements in those parts of the river where their effect will be the greatest, that is, where it is shallowest when its breadth greatly exceeds its depth, or where it is narrowest, (if its depth exceed the breadth, which is a very rare case), or in general, where the slope is the smallest for a short run.

The same general principles direct us in the method of embankments, for the prevention of floods, by enabling us to ascertain the heights necessary to be given to our banks. This will evidently depend, not only on the additional quantity of water which experience tells us a river brings down during its freshes, but also on the distance at which we place the banks from the natural banks of the river. This is a point where mistaken economy frequently defeats its own purpose. If we raise our embankment at some distance from the natural banks of the river, not only will a smaller height suffice, and consequently a smaller base, which will make a saving in the duplicate proportion of the height; but our works will be so much the more durable nearly, if not exactly, in the same proportion. For by thus enlarging the additional bed which we give to the swollen river, we diminish its velocity almost in the same proportion that we enlarge its channel, and thus diminish its power of ruining our works. Except, therefore, in the case of a river whose freshes are loaded with fine sand to destroy the turf, it is always proper to place the embankment at a considerable distance from the natural banks. Placing them at half the breadth of the stream from its natural banks, will nearly double its channel; and, except in the case now mentioned, the space thus detached from our fields will afford excellent pasture.

The limits of such a work as ours will not permit us to enter into any detail on the method of embankment. It would require a volume to give instructions as to the manner of founding, raising, and securing the dykes which must be raised, and a thousand circumstances which must be attended to. But a few general observations may be made, which naturally occur while we are considering the manner in which a river works in settling or altering its channel.

It must be remarked, in the first place, that the river will rise higher when embanked than it does while it was allowed to spread; and it is by no means easy to conclude to what height it will rise from the greatest height to which it has been observed to rise in its floods. When at liberty to expand over a wide valley, then it could only rise till it overflowed with a thickness or depth of water sufficient to produce a motion backwards into the valley quick enough to take off the water as fast as it was supplied; and we imagine that a foot or two would suffice in most cases. The best way for a prudent engineer will be to observe the utmost rise remembered by the neighbours in some gorge, where the river cannot spread out. Measure the increased section in this place, and at the same time recollect, that the water increases in a much greater proportion than the section; because an increase of the hydraulic mean depth produces an increase of velocity in the duplicate proportion of the depth nearly. But as this augmentation of velocity will obtain also between the embankments, it will be sufficiently exact to suppose that the section must be increased here nearly in the same proportion as at the gorge already mentioned. Neglecting this method of information, and regulating the height of our embankment by the greatest swell that has been observed in the plain, will assuredly make them too low, and render them totally useless.

A line of embankment should always be carried on by a strict concert of the proprietors of both banks through its whole extent. A greedy proprietor, by advancing his own embankment beyond that of his neighbours, not only exposes himself to risk by the working of the waters on the angles which this will produce, but exposes his neighbours also to danger, by narrowing the section, and thereby raising the surface and increasing the velocity, and by turning the stream athwart, and causing it to shoot against the opposite bank. The whole should be as much as possible in a line; and the general effect should be to make the course of the stream straighter than it was before. All bends should be made more gentle, by keeping the embankment further from the river in all convex lines of the natural bank, and bringing it nearer where the bank is concave. This will greatly diminish the action of the waters on the bankment, and ensure their duration. The same maxim must be followed in fencing any brook which discharges itself into the river. The bends given at its mouth to the two lines of embankment should be made less acute than those of the natural brook, although, by this means, two points of land are left out. And the opportunity should be embraced of making the direction of this transverse brook more sloping than before, that is, less athwart the direction of the river.

It is of great consequence to cover the outside of the dyke with very compact turf closely united. If it admit water, the interior part of the wall, which is always more porous, becomes drenched in water, and this water acts with its statical pressure, tending to burst the bank on the land-side, and will quickly shift it from its seat. The utmost care should therefore be taken to make it and keep it perfectly tight. It should be a continued fine turf, and every bare spot should be carefully covered with fresh sod; and rat-holes must be carefully closed up.

Of straighting or changing the Course of Rivers.

We have seen, that every bending of a river requires an additional slope in order to continue its train, or enable it to convey the same quantity of water without swelling in its bed. Therefore the effect of taking away any of these bends must be to sink the waters of the river. It is proper, therefore, to have it in our power to estimate these effects. It may be desirable to gain property, by taking away the sweeps of a very winding stream. But this may be prejudicial, by destroying the navigation on such a river. It may also hurt the proprietors below, by increasing the velocity of the stream, which will expose them to the risk of its overflowing, or of its destroying its bed, and taking a new course. Or this increase of velocity may be inconsistent with the regimen of the new channel, or at least require larger dimensions than we should have given it if ignorant of this effect.

Our principles of uniform motion enable us to answer every question of this kind which can occur; and M. de Buat proposes several problems to this effect. The regular solutions of them are complicated and difficult; and we do not think them necessary in this place, because they may all be solved in a manner not indeed so elegant, because indirect, but abundantly accurate, and easy to any person familiar with those which we have already considered.

We can take the exact level across all these sweeps, and thus obtain the whole slope. We can measure with accuracy the velocity in some part of the channel which is most remote from any bend, and where the channel itself has the greatest regularity of form. This will give us the expense or discharge of the river, and the mean depth connected with it. We can then examine whether this velocity is precisely such as is compatible with stability in the straight course. If it is, it is evident that if we cut off the bends, the greater slope which this will produce will communicate to the waters a velocity incompatible with the regimen suited to this soil, unless we enlarge the width of the stream, that is, unless we make the new channel more capacious than the old one. We must now calculate the dimensions of the channel which, with this increased slope, will conduct the waters with the velocity that is necessary. All this may be done by the foregoing problems; and we may easiest accomplish this by steps. First, suppose the bed the same with the old one, and calculate the velocity for the increased slope by the general formula. Then change one of the dimensions of the channel, so as to produce the velocity we want, which is a very simple process. And in doing this, the object to be kept chiefly in view is not to make the new velocity such as will be incompatible with the stability of the new bed.

Having accomplished this first purpose, we learn (in the very solution) how much shallower this channel with its greater slope will be than the former, while it discharges all the waters. This diminution of depth must increase the slope and the velocity, and must diminish the depth of the river, above the place where the alteration is to be made. How far it produces these effects may be calculated by the general formula. We then see whether the navigation will be hurt, either in the old river up the stream, or in the new channel. It is plain that all these points cannot be reconciled. We may make the new channel such, that it shall leave a velocity compatible with stability, and that it shall not diminish the depth of the river up the stream. But, having a greater slope, it must have a smaller mean depth, and also a smaller real depth, unless we make it of a very inconvenient form.

The same things, viewed in a different light, will show us what depression of waters may be produced by rectifying the course of a river in order to prevent its overflowing. And the process which we would recommend is the same with the foregoing. We apprehend it to be quite needless to measure the angles of rebound, in order to *compute* the slope which is *employed* for sending the river through the bend, with a view to supersede this by straighting the river. It is infinitely easier and more exact to measure the levels themselves, and then we know the effect of removing them.

Nor need we follow M. de Buat in solving problems for diminishing the slope and velocity, and deepening the channel of a river by bending its course. The expense of this would be in every case enormous; and the practices which we are just going to enter upon afford infinitely easier methods of accomplishing all the purposes which are to be gained by these changes.

Of Bars, Weirs, and Jetties, for raising the Surface of Rivers.

WE propose, under the article WATER-WORKS, to consider in sufficient practical detail all that relates to the construction and mechanism of these and other erections in water; and we confine ourselves, in this place, to the mere effect which they will produce on the current of the river.

We gave the name of *weir* or *bar* to a dam erected across a river for the purpose of raising its waters, whether in order to take off a draft for a mill or to deepen the channel. Before we can tell the effect which they will produce, we must have a general rule for ascertaining the relation between the height of the water above the lip of the weir or bar, and the quantity of water which will flow over.

First, then, with respect to a weir, represented in Fig. 19, 20. The latter figure more resembles their usual form, consisting of a dam of solid masonry, or built of timber, properly fortified with shoars and banks. On the top is set up a strong plank FR, called the waste-board, or waster, over which the water flows. This is brought to an accurate level, of the proper height. Such voiders are frequently made in the side of a mill-course, for letting the superfluous water run off. This is properly the *waster, voider*: it is also called an *offset*. The same observations will explain all these different pieces of practice. The following questions occur in course:

Prob. I. Given the length of an offset or wasteboard, made in the face of a reservoir of stagnant water, and the depth of its lip under the horizontal surface of the water, to determine the discharge, or the quantity of water which will run over in a second?

Let AB be the horizontal surface of the still water, and F the lip of the wasteboard. Call the depth BF under the surface h, and the length of the wasteboard l. *N. B.* The water is supposed to flow over into another basin or channel, so much lower that the surface HL of the water is lower, or at least not higher, than F.

If the water could be supported at the height BF, BF might be considered as an orifice in the side of a vessel. In which case, the discharge would be the same as if the whole water were flowing with the velocity acquired from the height $\frac{4}{9}$ BF, or $\frac{4}{9}\,h$. And if we suppose that there is no contraction at the orifice, the mean velocity would be $\sqrt{2g\,\frac{4}{9}\,h}, = \sqrt{772\frac{4}{9}\,h}$, in English inches per second. The area of this orifice is $l\,h$. Therefore the discharge would be $l\,h\,\sqrt{772\frac{4}{9}\,h}$, all being measured in inches. This is the usual theory; but it is not an exact representation of the manner in which the efflux really happens. The water cannot remain at the height BF; but in drawing towards the wasteboard from all sides, it forms a convex surface AIH, so that the point I, where the vertical drawn from the edge of the wasteboard meets the curve, is considerably lower than B. But as all the mass above F is supposed perfectly fluid, the pressure of the incumbent water is propagated, in the opinion of M. de Buat, to the filament passing over at F without any diminution. The same may be said of any filament between F and I. Each tends, therefore, to move in the same manner as if it were really impelled through an orifice in its place. Therefore the motions through every part of the line or plane IF are the same as if the water were escaping through an orifice IF, made by a sluice let down on the water, and keeping up the water of the reservoir to the level AB. It is beyond a doubt (says he) that the height IF must depend on the whole height BF, and that there must be a certain determined proportion between them. He does not attempt to determine this proportion theoretically, but says, that his experiments ascertain it with great precision to be the proportion of one to two, or that IF is always one-half of BF. He says, however, that this determination was not by an immediate and direct measurement; he concluded it from the comparison of the quantities of water discharged under different heights of the water in the reservoir.

We cannot help thinking that this reasoning is very defective in several particulars. It cannot be inferred, from the laws of hydrostatical pressure, that the filament at I is pressed forward with all the weight of the column BI. The particle I is really at the surface; and considering it as making part of the surface of a running stream, it is subjected to hardly any pressure, any more than the particles on the surface of a cup of water held in the hand, while it is carried round the axis of the earth and round the sun. Reasoning according to his own principles, and availing himself of his own discovery, he should say, that the particle at I has an accelerating force depending on its slope only; and then he should have endeavoured to ascertain this slope. The motion of the particle at I has no immediate connection with the pressure of the column BI; and if it had, the motion would be extremely different from what it is: for this pressure alone would give it the velocity which M. Buat assigns it. Now it is already passing through the point I with the velocity which it has acquired in descending along the curve AI; and this is the real state of the case. The particles are passing through with a velocity already acquired by a sloping current; and they are accelerated by the hydrostatical pressure of the water above them. The internal mechanism of these motions is infinitely more complex than M. Buat here supposes; and on this supposition he very nearly abandons the theory which he has so ingeniously established, and adopts the theory of Guglielmini which he had exploded. At the same time, we think that he is not much mistaken when he asserts, that the motions are nearly the same as if a sluice had been let down from the surface to I. For the filament which passes at I has been gliding down a curved surface, and has not been exposed to any friction. It is perhaps the very case of hydraulics, where the obstructions are the smallest; and we should therefore expect that its motion will be the least retarded.

We have therefore no hesitation in saying, that the filament at I is in the very state of motion which the theory would assign to it if it were passing under a sluice, as M. Buat supposes. And with respect to the inferior filaments, without attempting the very difficult task of investigating their motions, we shall just say, that we do not see any reason for supposing that they will move slower than our author supposes. Therefore, though we reject his theory, we admit his experimental proposition in general; that is, we admit that the *whole* water which passes through the plane IF moves with the velocity (though not in the same direction) with which it would have run through a sluice of the same depth; and we may proceed with his determination of the quantity of water discharged.

If we make BC the axis of a parabola BEGK, the velocities of the filaments passing at I and F will be represented by the ordinates IE and FG, and the discharge by the area IEGF. This allows a very neat solution of the problem. Let the quantity discharged per second be D, and let the whole height BF be h. Let 2 G be the quantity by which we must divide the square of the mean velocity, in order to have the producing height. This will be less than 2 g, the acceleration of gravity, on account of the convergency at the sides and the tendency to convergence at the lip F. We formerly gave for its measure 726 inches, instead of 772, and said that the inches discharged per second from an orifice of one inch were 26,49, instead of 27,78. Let x be the distance of any filament from the horizontal line AB. An element of the orifice, therefore, (for we may give it this name) is $l\dot{x}$. The velocity of this element is $\sqrt{2\,\mathrm{G}\,x}$, or $\sqrt{2\,\mathrm{G}} \times \sqrt{x}$. The discharge from it is $l\sqrt{2\mathrm{G}}\,x^{\frac{1}{2}}\,\dot{x}$, and the fluent of this, or $\mathrm{D} = \int l\sqrt{2\,\mathrm{G}}\,x^{\frac{1}{2}}\,\dot{x}$, which is $\frac{2}{3}\,l\sqrt{2\,\mathrm{G}}\,x^{\frac{3}{2}} + \mathrm{C}$. To determine the constant quantity C, observe that M. de Buat found by experiment that BI was in all cases $\frac{1}{2}$ BF. Therefore D must be nothing when $x = \frac{1}{2}\,h$; consequently $\mathrm{C} = -\frac{2}{3}\,l\sqrt{2\,\mathrm{G}}\left(\frac{h}{2}\right)^{\frac{3}{2}}$, and the completed fluent will be

$$\mathrm{D} = \tfrac{2}{3}\,l\sqrt{2\,\mathrm{G}}\left(x^{\frac{3}{2}} - \left(\tfrac{h}{2}\right)^{\frac{3}{2}}\right).$$

Now make $x = h$, and we have

$$\mathrm{D} = \tfrac{2}{3}\,l\sqrt{2\mathrm{G}}\left(h^{\frac{3}{2}} - \left(\tfrac{h}{2}\right)^{\frac{3}{2}}\right) = \tfrac{2}{3}\,l\sqrt{2\mathrm{G}}\left(1 - \left(\tfrac{1}{2}\right)^{\frac{3}{2}}\right)h^{\frac{3}{2}}.$$

But $1 - \left(\frac{1}{2}\right)^{\frac{3}{2}} = 0{,}64645$, and $\frac{2}{3}$ of this is 0,431: Therefore, finally,

$$\mathrm{D} = 0{,}431\,(\sqrt{2\,\mathrm{G}}\,h^{\frac{3}{2}} \times l.).$$

If we now put 26,49 or 26½ for $\sqrt{2\,\mathrm{G}}$, or the velocity with which a head of water of one inch will impel the water over a weir, and multiply this by 0,431, we get the following quantity, 11,4172, or, in numbers of easy recollection, 11½, for the cubic inches of water per second, which runs over every inch of a wasteboard when the edge of it is one inch below the surface of the reservoir; and this must be multiplied by $h^{\frac{3}{2}}$, or by the square root of the cube of the head of water. Thus let the edge of the wasteboard be four inches below the surface of the water. The cube of this is 64, of which the square root is eight. Therefore a wasteboard of this depth under the surface, and three feet long, will discharge every second 8 × 36 × 11¼ cubic inches of water, or 1 $\frac{8}{10}$ cubic feet, English measure.

The following comparisons will show how much this theory may be depended on. Column 1. shews the depth of the edge of the board under the surface; 2, shows the discharge by theory; and 3, the discharge actually observed. The length of the board was 18⅓ inches. N. B.—The number in M. Buat's experiments are here reduced to English measure:

D.	D. Theor.	D. Exp.	E.
1,778	506	524	28,98
3,199	1222	1218	69,83
4,665	2153	2155	123,03
6,753	3750	3771	214,29

The last column is the cubic inches discharged in a second by each inch of the wasteboard. The correspondence is undoubtedly very great. The greatest error is in the first, which may be attributed to a much smaller lateral contraction under so small a head of water.

But it must be remarked, that the calculation proceeds on two suppositions. The height FI is supposed ½ of BF; and 2 G is supposed 726. It is evident, that by increasing the one and diminishing the other, nearly the same answers may be produced, unless much greater variations of *h* be examined. Both of these quantities are matters of considerable uncertainty, particularly the first; and it must be farther remarked, that this was not measured, but deduced from the uniformity of the experiments. We presume that M. Buat tried various values of G, till he found one which gave the *ratios* of discharge which he observed. We beg leave to observe, that in a set of numerous experiments which we had access to examine, BI was uniformly much less than ½; it was nearly $\frac{2}{7}$: and the quantity discharged was greater than what would result from M.

Buat's calculation. It was farther observed, that IF depended very much on the form of the wasteboard. When it was a very thin board of considerable depth, IF was very considerably greater than if the board was thick, or narrow, and set on the top of a broad dam-head, as in Fig. 20.

It may be proper to give the formula a form which will correspond to any ratio which experience may discover between BF and IF. Thus, let BI be $\frac{m}{n}$ BF. The formula will be $D = \frac{2}{3} l \sqrt{2G} \left(1 - \left(\frac{m}{n}\right)^{\frac{3}{2}}\right) h^{\frac{3}{2}}$

We presume, therefore, that the following table will be acceptable to practicable engineers, who are not familiar with such computations. It contains, in the first column, the depth in English inches from the surface of the stagnant water of a reservoir to the edge of the wasteboard. The second column is the cubic feet of water discharged in a minute by every inch of the wasteboard:

Depth.	*Discharge.*	*Depth.*	*Discharge.*
1	0,403	10	12,748
2	1,140	11	14,707
3	2,095	12	16,758
4	3,225	13	18,895
5	4,507	14	21,117
6	5,925	15	23,419
7	7,466	16	25,800
8	9,122	17	28,258
9	10,884	18	30,786

When the depth does not exceed four inches, it will not be exact enough to take proportional parts for the fractions of an inch. The following method is exact:

If they be odd quarters of an inch, look in the table for as many inches as the depth contains quarters, and take the eighth part of the answer. Thus, for $3\frac{3}{4}$ inches, take the eighth part of 23,410, which corresponds to 15 inches. This is 2,927.

If the wasteboard is not on the face of a dam, but in a running stream, we must augment the discharge by multiplying the section by the velocity of the stream. But this correction can seldom occur in practice; because, in this case, the discharge is previously known; and it is h that we want; which is the object of the next problem.

$\frac{1}{16}$

We only beg leave to add, that the experiments which we mention as having been already made in this country, give a result somewhat greater than this table, viz. about $\frac{1}{16}$. Therefore, having obtained the answer by this table, add to it its 16th part, and we apprehend that it will be extremely near the truth.

When, on the other hand, we know the discharge over a wasteboard, we can tell the depth of its edge under the surface of the stagnant water of the reservoir, because we have $h = \left(\frac{D}{11\frac{1}{3}\, l}\right)^{\frac{2}{3}}$ very nearly.

We are now in a condition to solve the problem respecting a weir across a river.

Prob. II. The discharge and section of a river being given, it is required to determine how much the waters will be raised by a weir of the whole breadth of the river, discharging the water with a clear fall, that is, the surface of the water in the lower channel being below the edge of the weir?

In this case we have $2\,G = 746$ nearly, because there will be no contraction at the sides when the weir is the whole breadth of the river. But further, the water is not now stagnant, but moving with the velocity $\frac{D}{S}$, S being the section of the river.

Therefore let a be the height of the weir from the bottom of the river, and h the height of the water above the edge of the weir. We have the velocity with which the water approaches the weir $= \frac{D}{l(a+h)}$, l being the length of the weir or breadth of the river. Therefore the height producing the primary mean velocity is $\left(\frac{D}{l\sqrt{2}\,g(a+h}\right)^2$. The equation given a little ago will have $h = \left(\frac{D}{0,431\, l \sqrt{2\,G}}\right)^{\frac{2}{3}}$, when the water above the weir is stagnant. Therefore, when it is already moving with the velocity $\frac{D}{la+h}$, we shall have $h = \left(\frac{D}{0,431\sqrt{2G}}\right)^{\frac{2}{3}} - \left(\frac{D}{l\sqrt{2g}\,(a+h}\right)^2$. It would be very troublesome to solve this equation regularly, because the unknown quantity h is found in the second term of the answer. But we know that the height producing the velocity above the weir is very small in comparison of h and of a, and, if only estimated roughly, will make a very insensible change in the value of h; and, by repeating the operation, we can correct this value, and obtain h to any degree of exactness.

To illustrate this by an example. Suppose a river, the section of whose stream is 150 feet, and that it discharges 174 cubic feet of water in a second; how much will the waters of this river be raised by a weir of the same width, and 3 feet high?

Suppose the width to be 50 feet. This will give 3 feet for the depth; and we see that the water will have a clear fall, because the lower stream will be the same as before.

The section being 150 feet, and the discharge 174, the mean velocity is $\frac{174}{150}$, $= 1,16$ feet, $= 14$ inches nearly, which requires the height of $\frac{1}{4}$ of an inch very nearly. This may be taken for the second term of the value of h. Therefore $h = \left(\frac{D}{0,431\sqrt{2\,G}\,l}\right)^{\frac{2}{3}} - \frac{1}{4}$. Now $\sqrt{2\,G}$ is, in the present case, = 27,818; l is 600, and D is 174 × 1728, = 300672. Therefore $h = 12{,}192 - 0{,}25, = 11{,}942$. Now correct this value of h, by correcting the second term; which is ¼ of an inch, instead of $\left(\frac{D}{\sqrt{2g}\,l(a+h)}\right)^2$, or 0,141. This will give us $h = 12{,}192 - 0{,}141, = 12{,}051$, differing from the first value about $\frac{1}{10}$ of an inch. It is needless to carry the approximation farther. Thus we see that a weir, which dams up the whole of the former current of three feet deep, will only raise the waters of this river one foot.

The same rule serves for showing how high we ought to raise this weir in order to produce *any given rise* of the waters, whether for the purposes of navigation, or for taking off a draft to drive mills, or for any other service; for if the breadth of the river remain the same, the water will still flow over the weir with nearly the same depth. A very small and hardly perceptible difference will indeed arise from the diminution of slope occasioned by this rise, and a consequent diminution of the velocity with which the river approaches the weir. But this difference must always be a small fraction of the second term of our answer; which term is itself very small: and even this will be compensated, in some degree, by the freer fall which the water will have over the weir.

If the intended weir is not to have the whole breadth of the river (which is seldom necessary even for the purposes of navigation), the waters will be raised higher by the same height of the wasteboard. The calculation is precisely the same for this case. Only in the second term, which gives the head of water corresponding to the velocity of the river, l must still be taken for the whole breadth of the river, while in the first term l is the length of the wasteboard. Also $\sqrt{2G}$ must be a little less, on account of the contractions at the ends of the weir, unless these be avoided by giving the masonry at the ends of the wasteboard a curved shape on the upper side of the wasteboard. This should not be done when the sole object of the weir is to raise the surface of the waters. Its effect is but trifling at any rate, when the length of the wasteboard is considerable, in proportion to the thickness of the sheet of water flowing over it.

The following comparisons of this rule with experiment will give our readers some notion of its utility.

Discharge of the Weir per Second.	Head producing the velocity at the Weir.	Head producing the Velocity above it.	Calculated Height of the River above the Wasteboard.	Observed Height.
Inches.	Inches.	Inches.	Inches.	Inches.
3888	7,302	0,625	6,677	6,583
2462	5,385	0,350	5,035	4,750
1112	3,171	0,116	3,055	3,166
259	1,201	0,0114	1,189	1,250

It was found extremely difficult to measure the exact height of the water in the upper stream above the wasteboard. The curvature AI extended several feet up the stream. Indeed there must be something arbitrary in this measurement, because the surface of the stream is not horizontal. The deviation should be taken, not from a horizontal plane, but from the inclined surface of the river.

It is plain that a river cannot be fitted for continued navigation by WEIRS. These occasion interruptions; but a few inches may sometimes be added to the waters of a river by a BAR, which may still allow a flat-bottomed lighter or a raft to pass over it. This is a very frequent practice in Holland and Flanders; and a very cheap and certain conveyance of goods is there obtained by means of streams which we would think no better than boundary ditches, and unfit for every purpose of this kind. By means of a bar the water is kept up a very few inches, and the stream has free course to the sea. The shoot over the bar is prevented by means of another bar placed a little way below it, lying flat in the bottom of the ditch, but which may be raised up on hinges. The lighterman makes his boat fast to a stake immediately above the bar, raises the lower bar, brings over his boat, again makes it fast, and, having laid down the other bar again, proceeds on his journey. This contrivance answers the end of a lock at a very trifling expense; and though it does not admit of what we are accustomed to call navigation, it gives a very sure conveyance, which would otherwise be impossible. When the waters can be raised by bars, so that they may be drawn off for machinery or other purposes, they are preferable to weirs, because they do not obstruct floating with rafts, and are not destroyed by the ice.

Prop. III. Given the height of a bar, the depth of water both above and below it, and the width of the river, to determine the discharge?

This is by no means so easily solved as the discharge over a weir, and we cannot do it with the same degree of evidence. We imagine, however, that the following observations will not be very far from a true account of the matter :

We may first suppose a reservoir LFBM (Fig. 21.) of stagnant water, and that it has a wasteboard of the height CB. We may then determine, by the foregoing problems, the discharge through the plane EC. With respect to the discharge through the part CA, it should be equal to this product of the part of the section by the velocity corresponding to the fall EC, which is the difference of the heights of water above and below the bar; for, because the difference of E a and C a is equal to EC, every particle a of water in the plane CA is pressed in the direction of this stream with the same force, *viz.* the weight of the column EC. The sum of these discharges should be the whole discharge over the bar; but since the bar is set up across a running river, its discharge must be the same with that of the river. The water of the river, when it comes to the place of the bar, has acquired some velocity by its slope or other causes, and this corresponds to some height FE. This velocity, multiplied by the section of the river, having the height EB, should give a discharge equal to the discharge over the bar.

To avoid this complication of conditions, we may first compute the discharge of the bar in the manner now pointed out, without the consideration of the previous velocity of the stream. This discharge will be a little too small. If we divide it by the section FB, it will give a primary velocity too small, but not far from the truth. Therefore we shall get the height FE, by means of which we shall be able to determine a velocity intermediate between DG and CH, which would correspond to a weir, as also the velocity CH, which corresponds to the part of the section CA, which is wholly under water. Then we correct all these quantities by repeating the operation with them instead of our first assumptions.

M. Buat found this computation extremely near the truth, but in all cases a little greater than observation exhibited.

We may now solve the problem in the most general terms.

PROB. IV. Given the breadth, depth, and the slope of a river, if we confine its passage by a bar or weir of a known height and width, to determine the rise of the waters above the bar.

The slope and dimensions of the channel being given, our formula will give us the velocity and the quantity of water discharged. Then, by the preceding problem, find the height of water above the wasteboard. From the sum of these two heights deduct the ordinary depth of the river. The remainder is the rise of the waters. For example:

Let there be a river whose ordinary depth is 3 feet, and breadth 40, and whose slope is 1½ in 100 fathoms, or $\frac{1}{4800}$. Suppose a weir on this river 6 feet high, and 18 feet wide.

We must first find the velocity and discharge of the river in its natural state, we have $l = 400$ inches, $h = 36$, $\frac{1}{s} = \frac{1}{4800}$. Our formula of uniform motion gives $V = 28,45$, and $D = 405216$ cubic inches.

The contraction obtains here on the three sides of the orifice. We may therefore take $\sqrt{2G} = 26,1$. *N. B.* This example is M. Buat's, and all the measures are French. We have also a (the height of the weir) 72, and $2g = 724$. Therefore the equation $h = \left(\frac{D}{0,431\sqrt{2G}\,l}\right)^{\frac{2}{3}} - \left(\frac{D}{l\sqrt{2g}\,(a+h)}\right)^2$ becomes 30,182. Add this to the height of the weir, and the depth of the river above the sluice is 102,182 inches, = 8 feet and 6,182 inches. From this take 3 feet, and there remain 5 feet and 6,182 inches for the rise of the waters.

There is, however, an important circumstance in this rise of the waters, which must be distinctly understood before we can say what are the interesting effects of this weir. This swell extends, as we all know, to a considerable distance up the stream, but is less sensible as we go away from the weir. What is the distance to which the swell extends, and what increase does it produce in the depth at different distances from the weir?

If we suppose that the slope and the breadth of the channel remain as before, it is plain, that as we come down the stream from that point where the swell is insensible, the depth of the channel increases all the way to the dam. Therefore, as the same quantity of water passes through every section of the river, the velocity must diminish in the same proportion (very nearly) that the section increases. But this being an open stream, and therefore the velocity being inseparably connected with the slope of the surface, it follows, that the slope of the surface must diminish all the way from that point where the swell of the water is insensible to the dam. The surface, therefore, cannot be a simple inclined plane, but must be concave upwards, as represented in Fig. 22, where FKLB represents the channel of a river, and FB the surface of the water running in it. If this be kept up to A by a weir AL, the surface will be a curve FIA, touching the natural surface F at the beginning of the swell, and the line AD which touches it in A will have the slope S corresponding to the velocity which the waters have immediately before going over the weir. We know this slope, because we are supposed to know the discharge of the river and its slope and other circumstances before barring it with a dam; and we know the height of the dam H, and therefore the new velocity at A, or immediately above A, and consequently the slope S. Therefore, drawing the horizontal lines DC, AG, it is plain that CB and CA will be the primary slope of the river, and the slope S corresponding to the velocity in the immediate neighbourhood of A, because these verticals have the same horizontal distance DC. We have therefore $CB : CA = S : s$ very nearly, and $S - s : s = CB - CA : CA, = AB$ (nearly) $: CA$. Therefore $CA = \frac{AB \times s}{S - s}, = \frac{H s}{S - s}$. But $DA = CA \times S$, by our definition of slope; therefore $DA = \frac{H.S.s}{S - s}$.

This is all that we can say with precision of this curve. M. Buat examined what would result from supposing it an arch of a circle. In this case we should have $DA = DF$, and AF very nearly equal to 2 AD: and as we can thus find AD, we get the whole length FIA of the swell, and also the distances of any part of the curve from the primitive surface FB of the river; for these will be very nearly in the duplicate proportion of their distances from

F. Thus ID will be $\frac{1}{4}$ of AB, &c. Therefore we should obtain the depth I *d* of the stream in that place. Getting the depth of the stream, and knowing the discharge, we get the velocity, and can compare this with the slope of the surface at I. This should be the slope of that part of the arch of the circle. Making this comparison, he found these circumstances to be incompatible. He found that the section and swell at I, corresponding to an arch of a circle, gave a discharge nearly $\frac{1}{4}$th too great (they were as 405216 to 492142). Therefore the curve is such, that AD is greater than DF, and that it is more incurvated at F than at A. He found, that making DA to DF as 10 to 9, and the curve FIA an arch of an ellipse whose longer axis was vertical, would give a very nice correspondence of the sections, velocities, and slopes. The whole extent of the swell therefore can never be double of AD, and must always greatly surpass AD; and these limits will do very well for every practical question. Therefore making DF $\frac{9}{10}$ *of* AD, and drawing the chord AD, and making DI $\frac{1}{2}$ *of Di*, we shall be very near the truth. Then we get the swell with sufficient precision for any point H between F and D, by making $FD^2 : FH^2 = ID : Hh$; and if H is between D and A, we get its distance from the tangent DA by a similar process.

It only remains to determine the swell produced in the waters of a river by the erection of a bridge or cleaning sluice which contracts the passage. This requires the solution of

Prob. V. Given the depth, breadth, and slope of a river, to determine the swell occasioned by the piers of a bridge or sides of a cleaning sluice, which contract the passage by a given quantity, for a given length of channel.

This swell depends on two circumstances:

1. The whole river must pass through a narrow space, with a velocity proportionably increased; and this requires a certain head of water above the bridge.

2. The water, in passing the length of the piers with a velocity greater than that corresponding to the primary slope of the river, will require a greater slope in order to acquire this velocity.

Let V be the velocity of the river before the erection of the bridge, and K the quotient of the width of the river divided by the sum of the widths between the piers. If the length of the piers, or their dimension in the direction of the stream, is not very great, KV will nearly express the velocity of the river under the arches; and if we suppose for a moment the contraction (in the sense hitherto used) to be nothing, the height producing this velocity will be $\frac{K^2V^2}{2g}$. But the river will not rise so high, having already a slope and velocity before getting under the arches, and the height corresponding to this velocity is $\frac{V^2}{2g}$; therefore the height for producing the augmentation of velocity is $\frac{K^2V^2}{2g}-\frac{V^2}{2g}$. But if we make allowance for contraction, we must employ a 2 G less than $2g$, and we must multiply the height now found by $\frac{2g}{2G}$. It will then become $\left(\frac{K^2V^2}{2g}-\frac{V^2}{2g}\right)\frac{2g}{2G}, =\frac{V^2}{2G}(K^2-1)$. This is that part of the swell which must produce the augmentation of velocity.

With respect to what is necessary for producing the additional slope between the piers, let p be the natural slope of the river (or rather the difference of level in the length of the piers) before the erection of the bridge, and corresponding to the velocity V; K^2p will very nearly express the difference of superficial level for the length of the piers, which is necessary for maintaining the velocity KV through the same length. The *increase* of slope therefore is

$K^2 p - p = p\,(K^2 - 1)$. Therefore the whole swell will be $\left(\frac{V^2}{2G} + p\right)\overline{K^2 - 1}$.

These are the chief questions or problems on this subject which occur in the practice of an engineer; and the solutions which we have given may in every case be depended on as very near the truth, and we are confident that the errors will never amount to one-fifth of the whole quantity. We are equally certain, that of those who call themselves engineers, and who, without hesitation, undertake jobs of enormous expense, not one in ten is able even to guess at the result of such operations, unless the circumstances of the case happen to coincide with those of some other project which he has executed, or has distinctly examined; and very few have the sagacity and penetration necessary for appreciating the effects of the distinguishing circumstances which yet remain. The society established for the encouragement of arts and manufactures could scarcely do a more important service to the public in the line of their institution, than by publishing in their Transactions a description of every work of this kind executed in the kingdom, with an account of its performance. This would be a most valuable collection of experiments and facts. The unlearned practitioner would find among them something which resembles in its chief circumstances almost any project which could occur to him in his business, and would tell him what to expect in the case under his management: and the intelligent engineer, assisted by mathematical knowledge, and the habit of classing things together, would frequently be able to frame general rules. To a gentleman qualified as was the Chevalier de Buat, such a collection would be inestimable, and might suggest a theory as far superior to his as he has gone before all other writers, WE shall conclude this article with some observations on the methods which may be taken for rendering small rivers and brooks fit for inland navigation, or at least for floatage. We get much instruction on this subject from what has been said concerning the swell produced in a river by weirs, bars, or any diminution of its former section. Our knowledge of the form which the surface of this swell affects, will furnish rules for spacing these obstructions in such a manner, and at such distances from each other, that the swell produced by one shall extend to the one above it.

If we know the slope, the breadth, and the depth of a river, in the droughts of summer, and have determined on the height of the flood-gates, or keeps, which are to be set up in its bed, it is evident that their stations are not matters of arbitrary choice, if we would derive the greatest possible advantage from them.

Some rivers in Flanders and Italy are made navigable in some sort by simple sluices, which, being shut, form magazines of water, which, being discharged by opening the gates, raises the inferior reach enough to permit the passage of the craft which are kept on it. After this momentary rise the keeps are shut again, the water sinks in the lower reach, and the lighters which were floated through the shallows are now obliged to draw into those parts of the reach where they can lie afloat till the next supply of water from above enables them to proceed. This is a very rude and imperfect method, and unjustifiable at this day, when we know the effect of locks, or at least of double gates. We do not mean to enter on the consideration of these contrivances, and to give the methods of their construction, in this place.* At present we confine ourselves to the single point of husbanding the different falls in the bed of the river, in such a manner that there may be everywhere a sufficient depth of water; and, in what we have to deliver on the subject, we shall take the form of an example to illustrate the application of the foregoing rules.

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* See the article WATERWORKS in this volume, and also the EDINBURGH ENCYCLOPÆDIA, article NAVIGATION INLAND, Vol. XV. for farther information on these points.

Suppose then a river 40 feet wide and 3 feet deep in the droughts of summer, with a slope of 1 in 4800. This, by the formula of uniform motion, will have a velocity V = 23½ inches per second, and its discharge will be 405216 cubic inches, or 234½ feet. It is proposed to give this river a depth not less than five feet in any place, by means of flood-gates of six feet high and 18 feet wide.

We first compute the height at which this body of 234½ cubic feet of water will discharge itself over the flood-gates. This we shall find by Prob. II. to be 30¼ inches, to which adding 72, the height of the gate, we have 102¼ for the whole height of the water above the floor of the gate; the primitive depth of the river being 3 feet, the rise or swell 5 feet 6¼ inches. In the next place, we find the range or sensible extent of this swell by Prob. I. and the observations which accompany it. This will be found to be nearly 9177 fathoms. Now since the primitive depth of the river is three feet, there is only wanted two feet of addition; and the question is reduced to the finding what point of the curved surface of the swell is two feet above the tangent plane at the head of the swell? or how far this point is from the gate? The whole extent being 9177 fathoms, and the deviations from the tangent plane being nearly in the duplicate ratio of the distances from the point of contact, we may institute this proportion 66½ : 24 = $9177^2 : 5526^2$. The last term is the distance (from the head of the swell) of that part of the surface which is two feet above the primitive surface of the river. Therefore 9177 — 5526, or 3651 fathoms, is the distance of this part from the flood-gate; and this is the distance at which the gates should be placed from each other. No inconvenience would arise from having them nearer, if the banks be high enough to contain the waters; but if they are farther distant, the required depth of water cannot be had without increasing the height of the gates; but if reasons of conveniency should induce us to place them nearer, the same depth may be secured by lower gates, and no additional height will be required for the banks. This is generally a matter of moment, because the raising the water brings along with it the chance of flooding the adjoining fields. Knowing the place where the swell ceases to be sensible, we can keep the top of the intermediate flood-gate at the precise height of the curved surface of the swell by means of the proportionality of the deviations from the tangent to the distances from the point of contact.

But this rule will not do for a gate which is at a greater distance from the one above it than the 3651 fathoms already mentioned. We know that a higher gate is required, producing a more extensive swell; and the one swell does not coincide with the other, although they may both begin from the same point A (Fig. 23.). Nor will the curves even be similar, unless the thickness of the sheet of water flowing over the gate be increased in the same ratio. But this is not the case; because the produce of the river, and therefore the thickness of the sheet of water, is constant.

But we may suppose them similar, without erring more than two or three decimals of an inch; and then we shall have $AF : AL = fF : DL$; from which, if we take the thickness of the sheet of water already calculated for the other gates, there will remain the height of the gate BL.

By following these methods, instead of proceeding by random guesses, we shall procure the greatest depth of water at the smallest expense possible.

But there is a circumstance which must be attended to, and which, if neglected, may in a short time render all our works useless. These gates must frequently be open in the time of freshes; and as this channel then has its natural slope increased in every reach by the great contraction of the section in the gates, and also rolls along a greater body of water, the action of the stream on its bed must be increased by the augmentation of velocity which these circumstances will produce; and although we may say that the general slope is necessarily secured by the cills of the flood-gates, which are paved with stone or covered with planks, yet this will not hinder this increased current from digging up the bottom in the intervals, undermining the banks, and lodging the mud and earth thus carried off in places where the current meets with any check. All these consequences will assuredly follow if the increased velocity is greater than what corresponds to the regimen relative to the soil in which the river holds on its course.

In order therefore to procure durability to works of this kind, which are generally of enormous expense, *the local circumstances must be most scrupulously studied.* It is not the ordinary hurried survey of an engineer that will free us from the risk of our navigation becoming very troublesome by the rise of the waters being diminished from their former quantity, and banks formed at a small distance below every sluice. We must attentively study the nature of the soil, and discover experimentally the velocity which is not inconsistent with the permanency of the channel. If this be not a great deal less than that of the river when accelerated by freshes, the regimen may be preserved after the establishment of the gate, and no great changes in the channel will be necessary: but if, on the other hand, the natural velocity of the river during its freshes greatly exceeds what is consistent with stability, we must enlarge the width of the channel, that we may diminish the hydraulic mean depth, and along with this the velocity. Therefore, knowing the quantity discharged during the freshes, divide it by the velocity of regimen, or rather by a velocity somewhat greater (for a reason which will appear by and by), the quotient will be the area of a new section. Then taking the natural slope of the river for the slope which it will preserve in this enlarged channel, and after the cills of the flood-gates have been fixed, we must calculate the hydraulic mean depth, and then the other dimensions of the channel. And, lastly, from the known dimensions of the channel and the discharge (which we must now compute), we proceed to calculate the height and the distances of the flood-gates, adjusted to their widths, which must be regulated by the room which may be thought proper for the free passage of the lighters which are to ply on the river. An example will illustrate the whole of this process.

Suppose then a small river having a slope of 2 inches in 1000 fathoms, or $\frac{1}{3600}$, which is a very usual declivity of such small streams, and whose depth in summer is 2 feet, but subject to floods which raise it to 9 feet. Let its breadth at the bottom be 18 feet, and the base of its slanting sides $\frac{2}{3}$ of their height. All of these dimensions are very conformable to the ordinary course of things. It is proposed to make this river navigable in all seasons by means of keeps and gates placed at proper distances; and we want to know the dimensions of a channel which will be permanent, in a soil which begins to yield to a velocity of 30 inches per second, but will be safe under a velocity of 24.

The primitive channel, having the properties of a rectangular channel, its breadth during the freshes must be $B =$ 30 feet, or 360 inches, and its depth h 9 feet, or 108 inches; therefore its hydraulic mean depth $d = \frac{B h}{B+2h} = 61,88$ inches. Its real velocity therefore, during the freshes, will be 38,9447 inches, and its discharge 1514169 cubic inches, or 876¼ cubic feet per second. We see therefore that the natural channel will not be permanent, and will be very quickly destroyed or changed by this great velocity. We have two methods for procuring stability, viz. diminishing the slope, or widening the bed. The first method will require the course to be lengthened in the proportion of 24^2 to 3988^2, or nearly of 36 to 100. The expense of this would be enormous. The second method will require the hydraulic mean depth to be increased nearly in the same proportion (because the velocities are nearly as $\frac{\sqrt{d}}{\sqrt{s}}$). This will evidently be much less costly, and, even to procure convenient room for the navigation, must be preferred.

We must now observe, that the great velocity, of which we are afraid, obtains only during the winter floods. If therefore we reduce this to 24 inches, it must happen that the autumnal freshes, loaded with sand and mud, will certainly deposite a part of it, and choke up our channel below the flood-gates. We must therefore select a mean velocity somewhat exceeding the regimen, that it may carry off these depositions. We shall take 27 inches, which will produce this effect on the loose mud without endangering our channel in any remarkable degree.

Therefore we have, by the theorem for uniform motion,

$$V = 27, = \frac{297\,(\sqrt{d} - 0,1)}{\sqrt{s} - L\sqrt{s + 1,6}} - 0,3(\sqrt{s} - 0,1).$$ Calculating the divisor of this formula, we find it $= 55,884$. Hence $$\sqrt{d} - 0,1 = \frac{27 \text{ inch.}}{\frac{297}{55,884} - 0,3} = 5,3843,$$ and therefore $d = 30\frac{1}{12}$. Having thus determined the hydraulic mean depth, we find the area S of the section by dividing the discharge 1514169 by the velocity 27. This gives us 56080,368. Then we get the breadth B by the formula formerly given, $B = \sqrt{\left(\frac{S}{2d}\right)^2 - 2S} + \frac{S}{2d}, =$

1802,296 inches, or 150,19 feet, and the depth $h =$ 31,115 inches.

With these dimensions of the section we are certain that the channel will be permanent; and the cills of the flood-gates being all fixed agreeable to the primitive slope, we need not fear that it will be changed in the intervals by the action of the current. The gates being all open during the freshes, the bottom will be cleared of all deposited mud.

We must now station the flood-gates along the new channel, at such distances that we may have the depth of water which is proper for the lighters that are to be employed in the navigation. Suppose this to be four feet. We must first of all learn how high the water will be kept in this new channel during the summer droughts There remained in the primitive channel only 2 feet, and the section in this case had 20 feet 8 inches mean width; and the discharge corresponding to this section and slope of $\frac{1}{8000}$ is, by the theorem of uniform motion, 130,849 cubic inches per second. To find the depth of water in the new channel corresponding to this discharge, and the same slope, we must take the method of approximation formerly exemplified, remembering that the discharge D is 130849, and the breadth B is 1760,8 at the bottom (the slant sides being $\frac{4}{3}$). These data will produce a depth of water $= 6\frac{1}{3}$ inches. To obtain four feet therefore behind any of the flood-gates, we must have a swell of $41\frac{2}{3}$ inches produced by the gate below.

We must now determine the width of passage which must be given at the gates. This will regulate the thickness of the sheet of water which flows over them when shut; and this, with the height of the gate, fixes the swell at the gate. The extent of this swell, and the elevation of every point of its curved surface above the new surface of the river, requires a combination of the height of swell at the flood-gate, with the primitive slope and the new velocity. These being computed, the stations of the gates may be assigned, which will secure four feet of water behind each in summer. We need not give these computations, having already exemplified them all with relation to another river.

This example not only illustrates the method of proceeding, so as to be ensured of success, but also gives us a precise instance of what must be done in a case which cannot but frequently occur. We see what a prodigious excavation is necessary in order to obtain permanency. We have been obliged to enlarge the primitive bed to about thrice its former size, so that the excavation is at least two-thirds of what the other method required. The expense, however, will still be vastly inferior to the other, both from the nature of the work and the quantity of ground occupied. At all events, the expense is enormous, and what could never be repaid by the navigation, except in a very rich and populous country.

There is another circumstance to be attended to.—The navigation of this river by sluices must be very desultory, unless they are extremely numerous, and of *small heights*. The natural surface of the swell being concave upwards, the additions made by its different parts to the primitive height of the river decrease rapidly as they approach to the place A (Fig. 23.), where the swell terminates; and three gates, each of which raises the water one foot when placed at the proper distance from each other, will raise the water much more than two gates at twice this distance, each raising the water two feet. Moreover, when the elevation produced by a flood-gate is considerable, exceeding a very few inches, the fall and current produced by the opening of the gate is such, that no boat can possibly pass up the river, and it runs imminent risk of being overset and sunk, in the attempt to go down the stream. This renders the navigation desultory. A number of lighters collect themselves at the gates, and wait their opening. They pass through as soon as the current becomes moderate. This would not, perhaps, be very hurtful in a regulated navigation, if they could then proceed on their voyage. But the boats bound up the river must stay on the upper side of the gate which they have just now passed, because the channel is now too shallow for them to proceed. Those bound down the river can only go to the next gate, unless it has been opened at a time nicely adjusted to the opening of the one above it. The passage downwards *may*, in many cases, be continued, by very intelligent and attentive lockmen, but the passage up *must* be exceedingly tedious. Nay, we may say, that *while* the passage downwards is continuous, it is but in a very few cases that the passage upward is practicable. If we add to these inconveniences the great danger of passage during the freshes, while all the gates are open, and the immense and unavoidable accumulations of ice, on occasion even of slight frosts, we may see that this method of procuring an inland navigation is amazingly expensive, desultory, tedious, and hazardous. It did not therefore merit, on its own account, the attention we have bestowed on it. But the discussion was absolutely necessary, in order to show what must be done in order to obtain effect and permanency, and thus to prevent us from engaging in a project which, to a person not duly and confidently informed, is so feasible and promising. Many professional engineers are ready, and with honest intentions, to undertake such tasks; and by avoiding this immense expense, and contenting themselves with a much narrower channel, they succeed (witness the old navigation of the river Mersey). But the work has no duration; and, not having been found very serviceable, its cessation is not matter of much regret.

It was not a very refined thought to change this imperfect mode for another free from most of its inconveniences. A boat was brought up the river, through one of these gates, only by raising the waters of the inferior reach, and depressing those of the upper: and it could not escape observation, that when the gates were far asunder, a vast body of water must be discharged before this could be done, and that it would be a great improvement to double each gate, with a very small distance between. Thus a very small quantity of water would fill the interval to the desired height, and allow the boat to come through; and this thought was the more obvious, from a similar practice having preceded it, viz. that of navigating a small river by means of double bars, the lowest of which lay flat in the bottom of the river, but could be raised up on hinges. We have mentioned this already; and it appears to have been an old practice, being mentioned by Stevinus in his valuable work on sluices, published about the beginning of the last century; yet no trace of this method is to be found of much older dates. It occurred, however, accidentally, pretty often in the flat countries of Holland and Flanders, which being the seat of frequent wars, almost every town and village was fortified with wet ditches, connected with the adjoining rivers. Stevinus mentions particularly the works of Condé, as having been long employed, with great ingenuity, for rendering navigable a very long stretch of the Scheldt. The boats were received into the lower part of the fosse, which was separated from the rest by a stone batardeau, serving to keep up the waters in the rest of the fosse, about eight feet. In this was a sluice and another dam, by which the boats could be taken into the upper fosse, which communicated with a remote part of the Scheldt by a long canal. This appears to be one of the earliest locks.*

In the first attempt to introduce this improvement in the navigation of rivers already kept up by weirs, which gave a partial and interrupted navigation, it was usual to avoid the great expense of the second dam and gate, by making the lock altogether detached from the river, within land, and having its basin parallel to the river, and communicating by one end with the river above the weir, and by the other end with the river below the weir, and having a flood-gate at each end.—This was a most ingenious thought; and it was a prodigious improvement, free from all the inconveniences of currents, ice, &c. &c. It was called a *Schlussel*, or lock, with considerable propriety; and this was the origin of the word *sluice*, and of our application of its translation *lock*. This practice being once introduced, it was not long before engineers found that a complete separation of the navigation from the bed of the river was not only the most perfect method for obtaining a sure, easy, and uninterrupted navigation, but that it was in general the most economical in its first construction, and subject to no risk of deterioration by the action of the current, which was here entirely removed. Locked canals, therefore, have almost entirely supplanted all attempts to improve the natural beds of rivers; and this is hardly ever attempted except in the flat countries, where they can hardly be said to differ from horizontal canals. We therefore close with these observations this article, and refer for the construction of canals and locks to the following article on WATER-WORKS.*

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* A very full History of Locks, &c. will be found in the EDINBURGH ENCYCLOPÆDIA, vol. XV. p. 298, Art. INLAND NAVIGATION.—ED.

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* Our readers will probably be pleased with the following list of authors who have treated professedly of the motions of rivers: Guglielmini *De Fluviis et Castellis Aquarum—Danubius Illustratus;* Grandi *De Castellis;* Zendrini *De Motu Aquarum;* Frisius *De Fluviis;* Lecchi *Idrostatica i Idraulica;* Michelotti *Sperienze Idrauliche;* Belidor's *Architecture Hydraulique;* Bossut *Hydrodynamique;* Buat *Hydraulique;* Silberschlag *Theorie des Fleuves; Lettres de* M. L'Epinasse *au P. Frisi Touchant sa Theorie des Fleuves; Tableau des principales Rivieres du Monde, par* Genetté; Stevins *sur les Ecluses; Traité des Ecluses, par* Boulard, *qui a remporté le Prix de l'Acad. de Lyons;* Bleiswyck *Dissertatio de Aggeribus;* Bossut *et* Viallet *sur la Construction des Digues;* Stevin *Hydrostatica;* Tielman van der Horst *Theatrum Machinarum Universale;* De La Lande *sur les Canaux de Navigation;* Racolta di Autori *chi Trattano del Moto dell' Acque,* 3 tom. 4to, Firenza, 1723.—This most valuable collection contains the writings of Archimedes, Albizi, Galileo, Castelli, Michelini, Borelli, Montanari, Viviani, Cassini, Guglielmini, Grandi, Manfredi, Picard, and Narduci; and an account of the numberless works which have been carried on in the embankment of the Po.

WATER-WORKS.

UNDER this name may be comprehended almost every hydraulic structure or contrivance; such as, canals, conduits, locks, mills, water-engines, &c. But they may be conveniently arranged under two general heads, 1*st*, Works which have for their object the conducting, raising, or otherwise managing, of water; and, 2*dly*, Works which derive their efficacy from the impulse or other action of water. The *first* class comprehends the methods of simply conducting water in aqueducts or in pipes for the supply of domestic consumption, or the working of machinery: it comprehends also the methods of procuring the supplies necessary for these purposes, by means of pumps, water, or fire-engines. It also comprehends the subsequent management of the water thus conducted, whether in order to make the proper distribution of it according to the demand, or to employ it for the purpose of navigation, by lockage, or other contrivances.—And in the prosecution of these things many subordinate problems will occur, in which practice will derive great advantages from a scientific acquaintance with the subject.

CLASS I.

1. *Of the conducting of Water.*

THIS is undoubtedly a business of great importance, and makes a principal part of the practice of the civil engineer: it is also a business so imperfectly understood, that we believe that very few engineers can venture to say, with tolerable precision, what will be the quantity of water which his work will convey, or what plan and dimensions of conduit will convey the quantity which may be proposed. For proof of this we shall only refer our readers to the facts mentioned in the article **Rivers**.

In that article we have given a sort of history of the progress of our knowledge in hydraulics, a branch of mechanical philosophy which seems to have been entirely unknown to the ancients. Even Archimedes, the author of almost all that we know in hydrostatics, seems to have been entirely ignorant of any principles by which he could determine the motion of water. The mechanical science of the ancients seems to have reached no farther than the doctrine of equilibrium among bodies at rest. Guglielmini first ventured to consider the motion of water in open canals and in rivers. Its motion in pipes had been partially considered in detached scraps by others, but not so as to make a body of doctrine. Sir Isaac Newton first endeavoured to render hydraulics susceptible of mathematical demonstration: but his fundamental proposition has not yet been freed from very serious objections; nor have the attempts of his successors, such as the Bernouillis, Euler, D'Alembert, and others, been much more successful: so that hydraulics may still be considered as very imperfect, and the general conclusions, which we are accustomed to receive as fundamental propositions, are not much better than matters of observation, little supported by principle, and therefore requiring the most scrupulous caution in the application of them to any hitherto untried case. When experiments are multiplied so as to include as great a variety of cases as possible, and when these are cleared of extraneous circumstances, and properly arranged, we must receive the conclusions drawn from them as the general laws of hydraulics. The experiments of the Abbe Bossut, narrated in his *Hydrodynamique*, are of the greatest value, having been made in the cases of most general frequency, and being made with great care. The greatest service, however, has been done by the Chevalier Buat, who saw the folly of attempting to deduce an accurate theory from any principles that we have as yet learned, and the necessity of adhering to such a theory as could be deduced from experiment alone, independent of any more general principles. Such a theory must be a just one, if the experiments are really general, unaffected by the particular circumstances of the case, and if the classes of experiment are sufficiently comprehensive to include all the cases which occur in the most important practical questions. Some principle was necessary, however, for connecting these experiments. The sufficiency of this principle was not easily ascertained. M. Buat's way of establishing this was judicious. If the principle is ill-founded, the results of its combination in cases of actual experiments must be irregular; but if experiments, seemingly very unlike, and in a vast variety of dissimilar cases, give a train of results which is extremely regular and consistent, we may presume that the principle, which in this manner harmonizes and reconciles things so unlike, is founded in the nature of things; and if this principle be such as is agreeable to our clearest notions of the internal mechanism of the motion of fluids, our presumption approaches to conviction.

Proceeding in this way, the Chevalier Buat has collected a prodigious number of facts, comprehending almost every case of the motion of fluids. He first classed them according to their resemblance in some one particular, and observed the differences which accompanied their differences in other circumstances; and by considering what could produce these differences, he obtained general rules, deduced from fact, by which these differences could be made to fall into a regular series. He then arranged all the experiments under some other circumstance of resemblance, and pursued the same method; and by following this out, he has produced a general proposition, which applies to the whole of this numerous list of experiments with a precision far exceeding our utmost hopes. This proposition is contained in the article RIVERS, and is there offered as one of the most valuable results of modern science.

We must, however, observe, that of this list of experiments there is a very large class, which is not direct, but requires a good deal of reflection to enable us to draw a confident conclusion; and this is in cases which are very frequent and important, viz. where the declivity is exceedingly small, as in open canals and rivers. The experiments were of the following forms: two large cisterns were made to communicate with each other by means of a pipe. The surfaces of the water in these cisterns were made to differ only by a small fraction of an inch: and it is supposed that the motion in the communicating pipe will be the same as in a very long pipe, or an open canal, having this very minute declivity. We have no difficulty in admitting the conclusion; but we have seen it contested, and it is by no means intuitive. This, however, need not occasion any hesitation in the adoption of M. Buat's general proposition, because the experiments which we are now criticising fall in precisely with the general train of the rest, and show no *general* deviation which indicate a fallacy in principle.

We apprehend it to be quite unnecessary to add much to what has been already delivered on the motion of waters in an open canal. Their *general progressive* motion, and consequently the quantity delivered by an aqueduct of any slope and dimension, are sufficiently determined; and all that is wanted is the tables which we promised in the article RIVERS, by which any person who understands common arithmetic may compute the quantity of water which will be delivered by the aqueduct, canal, conduit, or pipe. We therefore take this opportunity of inserting these tables, which have been computed for this article with great labour.*

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* All the following Tables, recomputed and greatly extended by Mr Lourie, will be found in the EDINBURGH ENCYCLOPÆDIA, vol. XI. p. 520, Art. HYDRODYNAMICS.—ED.

TABLE I.—*Logarithms of the Values of the Numerator of the Fraction* $\frac{307(\sqrt{d}-0,1)}{\sqrt{s}-\mathrm{L}\sqrt{s+1,6}}$ *for every Value of the Hydraulic mean Depth* d: *Also the Values of* $0,3\,(\sqrt{d}-0,1)$.

d.	Log. of $307(\sqrt{d}-0,1)$	$0,3 \times (\sqrt{d}-0,1)$	*d.*	Log. of $307(\sqrt{d}-0,1)$	$0,3 \times (\sqrt{d}-0,1)$
0,1	1.82208	0,06	4,9	2.81216	0,63
0,2	2.02786	0,1	6,0	2.81674	0,63
0,3	2.13753	0,13	5,1	2.82125	0,65
0,4	2.21343	0,16	5,2	2.82567	0,65
0,5	2.27040	0,18	5,3	2.83000	0,66
0,6	2.31618	0,2	5,4	2.83222	0,67
0,7	2.35441	0,22	5,5	2.83840	0,67
0,8	2.38719	0,24	5,6	2.84248	0,68
0,9	2.41588	0,25	5,7	2.84648	0,68
1,0	2.44138	0,27	5,8	2.85043	0,69
1,1	2.46431	0,28	5,9	2.85431	0,69
1,2	2.48518	0,3	6,0	2.85812	0,7
1,3	2.50426	0,31	6,1	2.86185	0,7
1,4	2.52185	0,32	6,2	2.86554	0,71
1,5	2.53818	0,34	6,3	2.86916	0,72
1,6	2.55345	0,35	6,4	2.87271	0,73
1,7	2.56769	0,36	6,5	2.87622	0,73
1,8	2.58112	0,37	6,6	2.87966	0,74
1,9	2.59381	0,38	6,7	2.88306	0,75
2,0	2.60580	0,39	6,8	2.88641	0,75
2,1	2.61713	0,4	6,9	2.88971	0,76
2,2	2.62803	0,41	7,0	2.89296	0,76
2,3	2.63839	0,42	7,1	2.89614	0,77
2,4	2.64827	0,44	7,2	2.89930	0,77
2,5	2.65772	0,45	7,3	2.90241	0,78
2,6	2.66681	0,45	7,4	2.90549	0,78
2,7	2.67556	0,46	7,5	2.90851	0,79
2,8	2.68395	0,47	7,6	2.91150	0,79
2,9	2.69207	0,48	7,7	2.91445	0,8
3,0	2.69989	0,49	7,8	2.91734	0,8
3,1	2.70743	0,5	7,9	2.92022	0,81
3,2	2.71472	0,51	8,0	2.92305	0,82
3,3	2.72181	0,52	8,1	2.92584	0,82
3,4	2.72866	0,53	8,2	2.92860	0,83
3,5	2.73531	0,53	8,3	2.93133	0,83
3,6	2.74178	0,54	8,4	2.93403	0,84
3,7	2.74805	0,55	8,5	2.93670	0,84
3,8	2.75417	0,56	8,6	2.93933	0,85
3,9	2.76009	0,56	8,7	2.94192	0,85
4,0	2.76589	0,57	8,8	2.94449	0,86
4,1	2.77153	0,58	8,9	2.94703	0,86
4,2	2.77704	0,59	9,0	2.94954	0,87
4,3	2.78240	0,59	9,1	2.95202	0,87
4,4	2.78765	0,6	9,2	2.95447	0,88
4,5	2.79277	0,6	9,3	2.95690	0,88
4,6	2.79779	0,61	9,4	2.95930	0,89
4,7	2.80269	0,62	9,5	2.96167	0,89
4,8	2.80747	8,63	9,6	2.96402	0,9

TABLE I.—CONTINUED.

d.	Log. of $307(\sqrt{d}-0,1)$	$0,3 \times (\sqrt{d}-0,1)$	d.	Log. of $307(\sqrt{d}-0,1)$	$0,3 \times (\sqrt{d}-0,1)$
9,7	2.96634	0,9	54	3.34738	2,17
9,8	2.96865	0,91	55	3.35143	2,19
9,9	2.97093	0,91	56	3.35539	2,21
10,	2.97319	0,92	57	3.35928	2,23
11	2.99454	0,97	58	3.36312	2,25
12	3.01401	1,01	59	3.36687	2,27
13	3.03189	1,05	60	3.37057	2,3
14	3.04843	1,09	61	3.37421	2,31
15	3.06383	1,13	62	3.37778	2,33
16	3.07820	1,17	63	3.38130	2,35
17	3.09170	1,21	64	3.38477	2,37
18	3.10441	1,24	65	3.38817	2,39
19	3.11644	1,28	66	3.39158	2,41
20	3.12783	1,31	67	3.39483	2,42
21	3.13867	1,34	68	3.39809	2,44
22	3.14899	1,38	69	3.40130	2,46
23	3.15885	1,41	70	3.40446	2,48
24	3.16828	1,44	71	3.40758	2,49
25	3.17734	1,47	72	3.41065	2,51
26	3.18601	1,5	73	3.41369	2,53
27	3.19438	1,53	74	3.41667	2,55
28	3.20243	1,56	75	3.41962	2,57
29	3.21020	1,58	76	3.42253	2,58
30	3.21770	1,61	77	3.42540	2,60
31	3.22495	1,64	78	3.42823	2,62
32	3.23196	1,67	79	3.43103	2,63
33	3.23877	1,69	80	3.43380	2,65
34	3.24537	1,72	81	3.43653	2,67
35	3.25176	1,74	82	3.43923	2,69
36	3.25799	1,77	83	3.44189	2,7
37	3.26404	1,79	84	3.44452	2,72
38	3.26993	1,82	85	3.44712	2,74
39	3.27566	1,84	86	3.44968	2,75
40	3.28125	1,87	87	3.45222	2,77
41	3.28669	1,89	88	3.45473	2,78
42	3.29201	1,91	89	3.45721	2,79
43	3.29720	1,93	90	3.45965	2,81
44	3.30227	1,95	91	3.46208	2,83
45	3.30722	1,98	92	3.46448	2,85
46	3.31207	2,00	93	3.46685	2,86
47	3.31681	2,03	94	3.46920	2,88
48	3.32145	2,05	95	3.47152	2,89
49	3.32599	2,07	96	3.47381	2,91
50	3.33043	2,09	97	3.47608	2,93
51	3.33480	2,11	98	3.47833	2,94
52	3.33908	2,13	99	3.48056	2,95
53	3.34327	2,15	100	3.48277	2,97

TABLE II.

Logarithms of the Values of the Denominator of the Fraction $\frac{307\,(\sqrt{d}-0,1)}{\sqrt{s}-L\sqrt{:+1,6}}$ *for every Value of the Slope s.*

s.	Log. of $\sqrt{s}-L\sqrt{s+1,6}$	*s.*	Log. of $\sqrt{s}-L\sqrt{s+1,6}$	*s.*	Log. of $\sqrt{s}-L\sqrt{s+1,6}$
1,0	9.71784	5,2	0.12108	9,4	0.27110
1,1	9.74210	5,3	0.12595	9,5	0.27387
1,2	9.76388	5,4	0.13061	9,6	0.27656
1,3	9.78376	5,5	0.13519	9,7	0.27921
1,4	9.80202	5,6	0.13970	9,8	0.28186
1,5	9.81882	5,7	0.14410	9,9	0.28450
1,6	9.83461	5,8	0.14844	10	0.28709
1,7	9.84930	5,9	0.15274	—	
1,8	9.86314	6,0	0.15697	11	0.31170
1,9	9.87622	6,1	0.16113	12	0.33425
2,0	9.88857	6,2	0.16522	13	0.35488
2,1	9.90031	6,3	0.16927	14	0.37420
2,2	9.91153	6,4	0.17322	15	0.39235
2,3	9.92267	6,5	0.17713	16	0.40926
2,4	9.93247	6,6	0.18099	17	0.42521
2,5	9.94231	6,7	0.18477	18	0.44028
2,6	9.95173	6,8	0.18854	19	0.45439
2,7	9.96085	6,9	0.19229	20	0.46776
2,8	9.96942	7,0	0.19584	21	0.48044
2,9	9.97818	7,1	0.19886	22	0.49262
3,0	9.98632	7,2	0.20298	23	0.50433
3,1	9.99427	7,3	0.20651	24	0.51546
3,2	0.00200	7,4	0.20997	25	0.52621
3,3	0.00945	7,5	0.21336	26	0.53656
3,4	0.01669	7,6	0.21674	27	0.54654
3,5	0.02373	7,7	0.22109	28	0.55606
3,6	0.03064	7,8	0.22335	29	0.56526
3,7	0.03733	7,9	0.22663	30	0.57415
3,8	0.04383	8,0	0.22982	31	0.58263
3,9	0.05015	8,1	0.23297	32	0.59095
4,0	0.05638	8,2	0.23611	33	0.59901
4,1	0.06245	8,3	0.23923	34	0.60692
4,2	0.06839	8,4	0.24229	35	0.61448
4,3	0.07412	8,5	0.24532	36	0.62180
4,4	0.07898	8,6	0.24832	37	0.62900
4,5	0.08533	8,7	0.25128	38	0.63599
4,6	0.09081	8,8	0.25422	39	0.64276
4,7	0.09615	8,9	0.25709	40	0.64933
4,8	0.10131	9,0	0.25995	41	0.65571
4,9	0.10644	9,1	0.26281	42	0.66200
5,0	0.11147	9,2	0.26560	43	0.66811
5,1	0.11635	9,3	0.26839	44	0.67413

TABLE II.—CONTINUED.

Logarithms of the Values of the Denominator of the Fraction $\frac{307\,(\sqrt{d}-1,0)}{\sqrt{s}-\mathrm{L}\sqrt{:+1,6}}$ *for every Value of the Slope s.*

s.	Log. of $\sqrt{s}-\mathrm{L}\sqrt{s+1,6}$.	*s.*	Log. of $\sqrt{s}-\mathrm{L}\sqrt{s+1,6}$	*s.*	Log. of $\sqrt{s}-\mathrm{L}\sqrt{s+1,6}$
45	0.67997	87	0.85034	380	1.21806
46	0.68574	88	0.85327	390	1.22435
47	0.69135	89	0.85618	400	1.23048
48	0.69683	90	0.85908	410	1.23647
49	0.70226	91	0.86189	420	1.24232
50	0.70749	92	0.86463	430	1.24805
51	0.71265	93	0.86741	440	1.25360
52	0.71767	94	0.87017	450	1.25903
53	0.72263	95	0.87286	460	1.26433
54	0.72746	96	0.87552	470	1.26951
55	0.73223	97	0.87818	480	1.27461
56	0.73695	98	0.88076	490	1.27957
57	0.74155	99	0.88338	500	1.28445
58	0.74601	100	0.88593	510	1.28923
59	0.75043	—		520	1.29391
60	0.75481	110	0.91014	530	1.29851
61	0.75906	120	0.93212	540	1.30300
62	0.76328	130	0.95236	550	1.30740
63	0.76745	140	0.97109	560	1.31172
64	0.77151	150	0.98843	570	1.31597
65	0.77456	160	1.00466	580	1.32015
66	0.77945	170	1.01983	590	1.32426
67	0.78333	180	1.03410	600	1.32830
68	0.78718	190	1.04751	610	1.33226
69	0.79092	200	1.06026	620	1.33614
70	0.79463	210	1.07237	630	1.33997
71	0.79824	220	1.08390	640	1.34373
72	0.80182	230	1.09489	650	1.34743
73	0.80536	240	1.10542	660	1.35108
74	0.80882	250	1.11553	670	1.35468
75	0.81231	260	1.12523	680	1.35823
76	0.81571	270	1.13453	690	1.36170
77	0.81908	280	1.14345	700	1.36513
78	0.82236	290	1.15204	710	1.36851
79	0.82562	300	1.16035	720	1.37185
80	0.82885	310	1.16838	730	1.37513
81	0.83206	320	1.17612	740	1.37839
82	0.83525	330	1.18363	750	1.38157
83	0.83835	340	1.19092	760	1.38471
84	0.84142	350	1.19803	770	1.38782
85	0.84442	360	1.20490	780	1.39089
86	0.84739	370	1.21158	790	1.39391

TABLE II.—CONTINUED.

Logarithms of the Values of the Denominator of the Fraction $\frac{307\,(\sqrt{d}-0{,}1)}{\sqrt{s}-\mathrm{L}\sqrt{:+1{,}6}}$ *for every Value of the Slope s.*

s.	Log. of $\sqrt{s}-\mathrm{L}\sqrt{s+1{,}6}$	s.	Log. of $\sqrt{s}-\mathrm{L}\sqrt{s+1{,}6}$	s.	Log. of $\sqrt{s}-\mathrm{L}\sqrt{s+1{,}6}$
800	1.39690	3100	1.71313	7300	1.90845
810	1.39985	3200	1.72042	7400	1.91154
820	1.40277	3300	1.72750	7500	1.91458
830	1.40564	3400	1.73435	7600	1.91757
840	1.40678	3500	1.74099	7700	1.92052
850	1.41128	3600	1.74746	7800	1.92344
860	1.41408	3700	1.75373	7900	1.92632
870	1.41683	3800	1.75984	8000	1.92916
880	1.41953	3900	1.76578	8100	1.93197
890	1.42220	4000	1.77159	8200	1.93475
900	1.42487	4100	1.77725	8300	1.93749
910	1.42746	4200	1.78277	8400	1.94020
920	1.43005	4300	1.78814	8500	1.94287
930	1.43263	4400	1.79339	8600	1.94551
940	1.43515	4500	1.79851	8700	1.94811
950	1.43464	4600	1.80352	8800	1.95069
960	1.44011	4700	1.80875	8900	1.95324
970	1.44254	4800	1.81321	9000	1.95576
980	1.44498	4900	1.81790	9100	1.95826
990	1.44737	5000	1.82249	9200	1.96073
1000	1.44976	5100	1.82699	9300	1.96317
		5200	1.83142	9400	1.96559
1100	1.47223	5300	1.83575	9500	1.96797
1200	1.49269	5400	1.84002	9600	1.97033
1300	1.51148	5500	1.84421	9700	1.97267
1400	1.52885	5600	1.84833	9800	1.97497
1500	1.54497	5700	1.85237	9900	1.97726
1600	1.56014	5800	1.85634	10000	1.97952
1700	1.57416	5900	1.86022	11000	2.00099
1800	1.58747	6000	1.86404	12000	2.02056
1900	1.60004	6100	1.86778	13000	2.03855
2000	1.61195	6200	1.87146	14000	2.05518
2100	1.62325	6300	1.87507	15000	2.07065
2200	1.63403	6400	1.87863	16000	2.08512
2300	1.64432	6500	1.88213	17000	2.09869
2400	1.65414	6600	1.88558	18000	2.11148
2500	1.66358	6700	1.88898	19000	2.12357
2600	1.67261	6800	1.89233	20000	2.13503
2700	1.68133	6900	1.89564	21000	2.14594
2800	1.68971	7000	1.89891	22000	2.15633
2900	1.69780	7100	1.90214	23000	2.16624
3000	1.70558	7200	1.90532	24000	2.17573

TABLE I. consists of three columns.—*Column* 1, entitled *d*, contains the hydraulic mean depths of any conduit in inches. This is set down for every 10th of an inch in the first 10 inches, that the answers may be more accurately obtained for pipes, the mean depth of which seldom exceeds three or four inches. The column is continued to 100 inches, which is fully equal to the hydraulic mean depth of any canal.

Column 2. contains the logarithms of the values of $\sqrt{d}-0{,}1$, multiplied by 307; that is, the logarithm of the numerator of the fraction $\frac{307\,(\sqrt{d}-0{,}1)}{\sqrt{s}-\mathrm{L}\sqrt{s+1{,}6}}$ given in the article RIVERS.

Column 2. contains the products of the values of $\sqrt{d}-0{,}1$ multiplied by 0,3.

TABLE II. consists of two columns.—*Column* 1, entitled *s*, contains the denominator of the fraction expressing the slope or declivity of any pipe or canal; that is, the quotient of its length divided by the elevation of one extremity above the other. Thus, if a canal of one mile in length be three feet higher at one end than the other, then *s* is $\frac{5280}{3}, = 1760$.

Column 2. contains the logarithms of the denominators of the above-mentioned fraction, or of the different values of the quantity $\sqrt{s-\mathrm{L}\sqrt{s+1{,}6}}$.

These quantities were computed true to the third decimal place. Notwithstanding this, the last figure in about a dozen of the first logarithms of each table is not absolutely certain to the nearest unit. But this cannot produce an error of 1 in 100,000.

Examples of the Use of these Tables.

Example 1. Water is brought into the city of Edinburgh in several mains. One of these is a pipe of five

$= \frac{5}{4}$

inches diameter. The length of the pipe is 14,637 feet; and the reservoir at Comiston is 44 feet higher than the reservoir into which it delivers the water on the Castle Hill.

Query, The number of Scotch pints which this pipe should deliver in a minute?

1. We have $d = \frac{5}{4}$, $= 1,25$ inches. The logarithm corresponding to this d, being nearly the mean between the logarithms corresponding to 1,2 and 1,3, is 2.49472.

2. We have $s = \frac{14637}{44}$, or 332,7. The logarithm corresponding to this in Table II. is had by taking proportional parts for the difference between the logarithms for $s = 330$ and $s = 340$, and is 1.18533.

3. From 2.49472
   Take 1.18533

Remains 1.30939, the logarithm of 20,385 inches.

4. In column 3. of Table I. opposite to $d = 1,2$ and $d = 1,3$ are 0,3 and 0,31, of which the mean is 0,305 inches, the correction for viscidity.

5. Therefore the velocity in inches *per* second is 20,385 — 0,305, or 20,08.

6. To obtain the Scotch pints *per* minute (each containing 103, 4 cubic inches), multiply the velocity by 60, and this product by 5^2, and this by 0,7854 (the area of a circle whose diameter is 1), and divide by 103,4. Or by logarithms,

Add the log. of 20,08 - -	1.30276
log. of 60″ - -	1.77815
log. of 5^2 or 25 - -	1.39794
log. of 0,7854 - -	9.89509
	4.37394
Subtract the log. of 103,4 - -	2.01451
Remains the log. of 228,8 pints	2.35943

Example 2. The canal mentioned in the article RIVERS was 18 feet broad at the surface, and 7 feet at the bottom. It was 4 feet deep, and had a declivity of 4 inches in a mile. *Query*, The mean velocity?

1. The slant side of the canal, corresponding to 4 feet deep and $5\frac{1}{2}$ projection, is 6,8 feet; therefore the border touched by the water is $6{,}8+7+6{,}8, = 20{,}6$. The area is $4\times\frac{18+7}{2}, = 50$ square feet. Therefore $d = \frac{50}{20{,}6}, = 2{,}427$ feet, or 29,124 inches. The logarithm corresponding to this in Table I. is 3.21113, and the correction for viscidity from the third column of the same Table is 1,58.

2. The slope is one-third of a foot in a mile, or one foot in three miles. Therefore s is 15,840. The logarithm corresponding to this is 2.08280.

3. From 3.21113
Subtract 2.08280

Remains 1.12833 = log. 13,438 of inches.
Subtract for viscidity 1.58

Velocity per second 11,858

This velocity is considerably smaller than what was observed by Mr Watt. And indeed we observe, that in the very small declivities of rivers and canals, the formula is a little different. We have made several comparisons with a formula which is essentially the same with Buat's, and comes nearer in these cases. Instead of taking the hyperbolic logarithm of $\sqrt{s+1{,}6}$, multiply its common logarithm by $2\frac{1}{4}$, or multiply it by 9, and divide the product by 4; and this process is vastly easier than taking the hyperbolic logarithm.

We have not, however, presumed to calculate tables on the authority of our own observations, thinking too respect-fully, of this gentleman's labours and observations. But this subject will, ere long, be fully established by a series of observations on canals of various dimensions and declivities, made by several eminent engineers during the execution of them. Fortunately M. Buat's formula is chiefly founded on observations on small canals; and is therefore most accurate in such works where it is most necessary, viz. in mill courses, and other derivations for working machinery.

We now proceed to take notice of a few circumstances which deserve attention, in the construction of canals, in addition to those delivered in the article RIVERS.

When a canal or aqueduct is brought off from a basin or larger stream, it ought always to be widened at the entry, if it is intended for drawing off a continued stream of water: for such a canal has a slope, without which it can have no current. Suppose it filled to a dead level to the farther end. Take away the bar, and the water immediately begins to flow off at that end. But it is some time before any motion is perceived at the head of the canal, during all which time the motion of the water is augmenting in every part of the canal; consequently the slope is increasing in every part, this being the sole cause of its stream. When the water at the entry *begins* to move, the slope is scarcely sensible there; but it sensibly steepens every moment with the increase of velocity, which at last attains its maximum relative to the slope and dimensions of the whole canal; and this regulates the depth of water in every point down the stream. When all has attained a state of permanency, the slope at the entry *remains* much greater than in any other part of the canal: for this slope must be such as will produce a velocity sufficient for supplying its TRAIN.

And it must be remembered, that the velocity which must be produced greatly exceeds the mean velocity corresponding to the train of the canal. Suppose that this is

25 inches. There must be a velocity of 80 inches at the surface, as appears by the Table in the article RIVERS. This must be produced by a real fall at the entry.

In every other part the slope is sufficient, if it merely serves to give the water (already in motion) force enough for overcoming the friction and other resistances. But at the entry the water is stagnant, if in a basin, or it is moving past laterally, if the aqueduct is derived from a river; and, having no velocity whatever in the direction of the canal, it must derive it from its slope. The water therefore which has acquired a permanent form in such an aqueduct, must necessarily take that form which exactly performs the offices requisite in its different portions. The surface remains horizontal in the basin, as at KC (Fig. 1.), till it comes near the entry of the canal AB, and there it acquires the form of an undulated curve CDE; and then the surface acquires an uniform slope EF, in the lower part of the canal, where the water is in train.

If this is a drain, the discharge is much less than might be produced by the same bed if this sudden slope could be avoided. If it is to be navigated, having only a very gentle slope in its whole length, this sudden slope is a very great imperfection, both by diminishing the depth of water, which might otherwise be obtained along the canal, and by rendering the passage of boats into the basin very difficult, and the coming out very hazardous.

All this may be avoided, and the velocity at the entry may be kept equal to that which forms the train of the canal, by the simple process of enlarging the entry. Suppose that the water could accelerate along the slopes of the canal, as a heavy body would do on a finely polished plane. If we now make the width of the entry in its different parts inversely proportional to the fictitious velocities in those parts, it is plain that the slope of the surface will be made parallel to that of the canal which is in train. This will require a form somewhat like a bell or speaking-trumpet, as may easily be shown by a mathematical discussion. It would, however, be so much evasated at the basin as to occupy much room, and it would be very expensive to make such an excavation. But we may, at a very moderate expense of money and room, make the increase of velocity at the entry almost insensible. This should always be done, and it is not all expense: for if it be not done, the water will undermine the banks on each side, because it is moving very swiftly, and will make an excavation for itself, leaving all the mud in the canal below. We may observe this enlargement at the entry of all natural derivations from a basin or lake. It is a very instructive experiment, to fill up this enlargement, continuing the parallel sides of the drain quite to the side of the lake. We shall immediately observe the water grow shallower in the drain, and its performance will diminish. Supposing the ditch carried on with parallel sides quite to the side of the basin, if we build two walls or dykes from the extremities of those sides, bending outwards with a proper curvature (and this will often be less costly than widening the drain), the discharge will be greatly increased. We have seen instances where it was nearly doubled.

The enlargement at the mouths of rivers is generally owing to the same cause. The tide of flood up the river produces a superficial slope opposite to that of the river, and this widens the mouth. This is most remarkable when the tides are high, and the river has little slope.

After this great fall at the entry of a canal, in which all the filaments are much accelerated, and the inferior ones most of all, things take a contrary turn. The water, by rubbing on the bottom and the sides, is retarded; and therefore the section must, from being shallow, become a little deeper, and the surface will be convex for some distance till all comes into train. When this is established, the filaments nearest the bottom and side are moving slowest, and the surface (in the middle especially) retains the greatest velocity, gliding over the rest. The velocity in the canal, and the depth of the section, adjust themselves in such a manner that the difference between the surface of the basin and the surface of the uniform section of the canal corresponds exactly to the velocity. Thus, if this be observed to be two feet in a second, the difference of height will be three-sixteenths of an inch.

All the practical questions that are of considerable importance respecting the motion of water in aqueducts, may be easily, though not elegantly, solved by means of the tables.

But it is to be remembered, that these tables relate only to uniform motion, that is, to water that is in train, and where the velocity suffers no change by lengthening the conduit, provided the slope remain the same. It is much more difficult to determine what will be the velocity, &c. in a canal of which nothing is given but the form, and slope, and depth of the entry, without saying how deep the water runs in it. And it is here that the common doctrines of hydraulics are most in fault, and unable to teach us how deep the water will run in a canal, though the depth of the basin at the entry be perfectly known. Between the part of the canal which is in train and the basin, there is an interval where the water is in a state of acceleration, and is afterwards retarded.

The determination of the motions in this interval is exceedingly difficult, even in a rectangular canal. It was one great aim of Mr Buat's experiments to ascertain this by measuring accurately the depth of the water. But he found that, when the slope was but a very few inches in the whole length of his canal, it was not in train for want of greater length; and when the slope was still less, the small fractions of an inch, by which he was to judge of the variations of depth, could not be measured with sufficient accuracy. It would be a most desirable point to determine the length of a canal, whose slope and other dimensions are given, which will bring it into train; and what is the ratio which will then obtain between the depth at the entry and the depth which will be maintained. Till this be done, the engineer cannot ascertain by a direct process what quantity of water will be drawn off from a reservoir by a given canal. But as yet this is out of our reach. Experiments, however, are in view which will promote the investigation.

But this and similar questions are of such importance, that we cannot be said to have improved hydraulics, unless we can give a tolerably precise answer. This we can do by a sort of retrograde process, proceeding on the principles of uniform motion established by the Chevalier Buat. We may suppose a train maintained in the canal, and then examine whether this train *can* be produced by any fall that is possible at the entry. If it can, we may be certain that it is so produced, and our problem is solved.

We shall now point out the methods of answering some chief questions of this kind.

Quest. 1. Given the slope s and the breadth w of a canal, and the height H of the surface of the water in the basin above the bottom of the entry, to find the depth h and velocity V of the stream, and the quantity of water Q which is discharged?

The chief difficulty is to find the depth of the stream where it is in train. For this end, we may simplify the hydraulic theorem of uniform motion of the article RIVER; making $V = \frac{\sqrt{Ngd}}{\sqrt{S}}$, where g is the velocity (in inches) acquired in a second by falling, d is the hydraulic mean depth, and $\sqrt{S}$ stands for $\sqrt{3} - L\sqrt{S + 1{,}6}$. N is a number to be fixed by experiment (see RIVER,) depending on the contraction or obstruction sustained at the entry of the canal, and it may in most common cases be taken $= 244$; so that $\sqrt{Ng}$ may be somewhat less than

307. To find it we may begin by taking for our depth of stream a quantity h, somewhat smaller than H the height of the surface of the basin above the bottom of the canal. With this depth, and the known width w of the canal, we can find the hydraulic depth d (River,) Then with $\sqrt{d}$ and the slope find V by the Table: make this $V = \frac{\sqrt{Ngd}}{\sqrt{S}}$. This gives $\sqrt{Ng} = \frac{V\sqrt{S}}{\sqrt{d}}$. This value of Ng is sufficiently exact; for a small error of depth hardly affects the hydraulic mean depth.

After this preparation, the expression of the mean velocity in the canal will be $\frac{\sqrt{Ng}\sqrt{\frac{wh}{w+2h}}}{\sqrt{S}}$. The height which will produce this velocity is $\frac{Ng}{2GS}\left(\frac{wh}{w+2h}\right)$. Now this is the slope at the entry of the canal which produces the velocity that is afterwards maintained against the obstructions by the slope of the canal. It is therefore $= H - h$. Hence we deduce $h = \frac{-\left(w\left(\frac{Ng}{2GS}+1\right)-2H\right)}{4}$ $+ \frac{\sqrt{8Hw+\left(w\left(\frac{Ng}{2GS}+1\right)-2H\right)^2}}{4}$. If there be no contraction at the entry, $g = G$ and $\frac{9}{2G} = \frac{1}{2}$.

Having thus obtained the depth h of the stream, we obtain the quantity of water by combining this with the width w and the velocity V.

But as this was but an approximation, it is necessary to examine whether the velocity V be possible. This is very easy. It must be produced by the fall $H - h$. We shall have no occasion for any correction of our first assumption, if h has not been extravagantly erroneous, because a small mistake in h produces almost the same variation in d. The test of accuracy, however, is, that h, together with the height which will produce the velocity V, must make up the whole height H. Assuming h too small leaves $H - h$ too great, and will give a small velocity V, which requires a small value of $H - h$. The error of $H - h$ therefore is always greater than the error we have committed in our first assumption. Therefore when this error of $H - h$ is but a trifle, such as one-fourth of an inch, we may rest satisfied with our answer.

Perhaps the easiest process may be the following: Suppose the whole stream in train to have the depth H. The velocity V obtained for this depth and slope by the Table requires a certain productive height u. Make $\sqrt{H} + u : H = H : h$, and h will be exceedingly near the truth. The reason is obvious.

Quest. 2. Given the discharge (or quantity to be furnished in a second) Q. The height H of the basin above the bottom of the canal, and the slope, to find the dimensions of the canal?

Let x and y be the depth and mean width. It is plain that the equation $\frac{Q}{x\,y} = \sqrt{2G}\,\sqrt{H - x}$ will give a value of y in terms of x. Compare this with the value of y obtained from the equation $\frac{Q}{x\,y} = \frac{\sqrt{Ng}}{\sqrt{S}}\sqrt{\frac{x\,y}{y + 2\,x}}$. This will give an equation containing only x and known quantities. But it will be very complicated, and we must have recourse to an approximation. This will be best understood in the form of an example.

Suppose the depth at the entry to be 18 inches, and the slope $\frac{1}{1000}$. Let 1200 cubic feet of water *per* minute be the quantity of water to be drawn off, for working machinery or any other purpose; and let the canal be supposed of the best form, recommended in the article River, where the base of the sloping side is four-thirds of the height.

The slightest consideration will show us that if $\frac{V^2}{744}$ be taken for the height producing the velocity, it cannot exceed 2 inches, nor be less than 1. Suppose it = 2, and therefore the depth of the stream in the canal to be 16 inches; find the mean width of the canal by the equation

$$w = \frac{Q}{h\,(\surd d - 0{,}1)\left(\frac{307}{\surd S} - 0{,}3\right)},$$ in which Q is 20 cubic feet (the 60th part of 1200), $\surd S$ is $= 28{,}153$, $= \sqrt{1000} - \text{L}\,\sqrt{1000 + 1{,}6}$, and $h = 16$. This gives $w = 5{,}52$ feet. The section $n = 7{,}36$ feet, and $V = 32{,}6$ inches. This requires a fall of 1,52 inches instead of 2 inches. Take this from 18, and there remains 16,48, which we shall find not to differ one-tenth of an inch from the exact depth which the water will acquire and maintain. We may therefore be satisfied with assuming 5,36 feet as the mean width, and 3,53 feet for the width at the bottom.

This approximation proceeds on this consideration, that when the width diminishes by a small quantity, and in the same proportion that the depth increases, the hydraulic mean depth remains the same, and therefore the velocity also remains, and the quantity discharged changes in the exact proportion of the section. Any minute error which may result from this supposition, may be corrected by increasing the fall producing the velocity in the proportion of the first hydraulic mean depth to the mean depth corresponding to the new dimensions found for the canal. It will now become 1,53, and V will be 32,72, and the depth will be 16,47. The quantity discharged being divided by V, will give the section = 7,335 feet, from which, and the new depth, we obtain 5,344 for the width.

This and the foregoing are the most common questions proposed to an engineer. We asserted with some confidence that few of the profession are able to answer them with tolerable precision. We cannot offend the professional gentlemen by this, when we inform them, that the Academy of Sciences at Paris were occupied during several months with an examination of a plan proposed by M. De Parcieux, for bringing the waters of the Yvette into Paris; and after the most mature consideration, gave in a report of the quantity of water which M. De Parcieux's aqueduct would yield, and that their report has been found erroneous in the proportion of at least 2 to 5: for the waters have been brought in, and exceed the report in this proportion. Indeed long after the giving in the report, M. Perronet, the most celebrated engineer in France, affirmed that the dimensions proposed were much greater than were necessary, and said, that an aqueduct of 5¼ feet wide, and 3¼ deep, with a slope of 15 inches in a thousand fathoms, would have a velocity of 12 or 13 inches *per* second, which would bring in all the water furnished by the proposed sources. The great diminution of expense occasioned by the alteration encouraged the community to undertake the work. It was accordingly begun, and a part executed. The water was found to run with a velocity of near 19 inches when it was 3½ feet deep. M. Perronet founded his computation on his own experience alone, acknowledging that he had no theory to instruct him. The work was carried no farther, it being found that the city could be supplied at a much smaller expense by steam-engines erected by Boulton and Watt. But the facts which occurred in the partial execution of the aqueduct are very valuable. If M. Perronet's aqueduct be examined by our general formula, s will be found $= \frac{1}{4800}$, and $d = 18,72$, from which we deduce the velocity $= 18\frac{2}{3}$, agreeing with the observation with astonishing precision.

The experiments at Turin by Michelotti on canals were very numerous, but complicated with many circumstances which would render the discussion too long for this place.

When cleared of these circumstances, which we have done with scrupulous care, they are also abundantly conformable to our theory of the uniform motion of running waters. But to return to our subject:

Should it be required to bring off at once from the basin a mill course, having a determined velocity for driving an under-shot wheel, the problem becomes easier, because the velocity and slope combined determine the hydraulic mean depth at once; and the depth of the stream will be had by means of the height which must be taken for the whole depth at the entry, in order to produce the required velocity.

In like manner, having given the quantity to be discharged, and the velocity and the depth at the entry, we can find the other dimensions of the channel; and the mean depth being fonnd, we can determine the slope.

When the slope of a canal is very small, so that the depth of the uniform stream differs but a little from that at the entry, the quantity discharged is but small. But a great velocity, requiring a great fall at the entry, produces a great diminution of depth, and therefore it may not compensate for this diminution, and the quantity discharged may be smaller. Improbable as this may appear, it is not demonstrably false; and hence we may see the propriety of the following

Question 3. Given the depth H at the entry of a rectangular canal, and also its width w, required the slope, depth, and velocity, which will produce the greatest possible discharge?

Let x be the unknown depth of the stream. $H - x$ is the productive fall, and the velocity is $\sqrt{2G}\ \sqrt{H - x}$. This multiplied by $w\,x$ will give the quantity discharged. Therefore $w\,x\,\sqrt{2G}\ \sqrt{H - x}$ must be made a maximum. The common process for this will give the equation $2H = 3x$, or $x = \frac{2}{3}H$. The mean velocity will be $\sqrt{2G}$ $\sqrt{\frac{1}{3}H}$; the section will be $\frac{2}{3}\,w\,H$, and the discharge =

$\frac{2}{3}\sqrt{2G}\, w\, H \sqrt{\frac{1}{3}H}$, and $d = \frac{\frac{2}{3} w H}{w + \frac{2}{3} H}$. With these data the slope is easily had by the formula for uniform motion.

If the canal is of the trapezoidal form, the investigation is more troublesome, and requires the resolution of a cubic equation.

It may appear strange that increasing the slope of a canal beyond the quantity determined by this problem can diminish the quantity of water conveyed. But one of these two things must happen; either the motion will not acquire uniformity in such a canal for want of length, or the discharge must diminish. Supposing, however, that it could augment, we can judge how far this can go. Let us take the extreme case, by making the canal vertical. In this case it becomes a simple weir or wasteboard. Now the discharge of a wasteboard is $\frac{2}{3}\sqrt{2G}\, w$ $(h^{\frac{3}{2}} - (\frac{1}{2}h)^{\frac{3}{2}}$. The maximum determined by the preceding problem is to that of the wasteboard of the same dimensions as $H\sqrt{\frac{1}{3}H} : H^{\frac{3}{2}} - (\frac{1}{2}H)^{\frac{3}{2}}$, or as $H\sqrt{\frac{1}{3}H}$: $H\sqrt{H} - \frac{1}{2}H\sqrt{\frac{1}{2}H}$, $= 5773 : 6465$, nearly $= 9 : 10$.

Having given the dimensions and slope of a canal, we can discover the relation between its expenditure and the time; or we can tell how much it will sink the surface of a pond in 24 hours, and the gradual progress of this effect; and this might be made the subject of a particular problem. But it is complicated and difficult. In cases where this is an interesting object, we may solve the question with sufficient accuracy, by calculating the expenditure at the beginning, supposing the basin kept full. Then, from the known area of the pond, we can tell in what time this expenditure will sink an inch; do the same on the supposition that the water is one-third lower, and that it is two-thirds lower (noticing the contraction of the surface of the pond occasioned by this abstraction of its waters). Thus we shall obtain three rates of diminution, from which we can easily deduce the desired relation between the expenditure and the time.

Aqueducts derived from a basin or river are commonly furnished with a sluice at the entry. This changes exceedingly the state of things. The slope of the canal may be precisely such as will maintain the mean velocity of the water which passes under the sluice ; in which case the depth of the stream is equal to that of the sluice, and the velocity is produced at once by the head of water above it. But if the slope is less than this, the velocity of the issuing water is diminished, and the water must rise in the canal. This must check the efflux at the sluice, and the water will be as it were stagnant above what comes through below it. It is extremely difficult to determine at what precise ope the water will begin to check the efflux. The contraction at the lower edge of the board hinders the water from attaining at once the whole depth which it acquires afterwards, when its velocity diminishes by the obstructions. While the regorging which these obstructions occasion does not reach back to the sluice, the efflux is not affected by it.—Even when it does reach to the sluice, there will be a less depth immediately behind it than farther down the canal, where it is in train ; because the swift-moving water which is next the bottom drags with it the regorged water which lies on it : but the canal must be rapid to make this difference of depth sensible. In ordinary canals, with moderate slopes and velocities, the velocity at the sluice may be safely taken as if it were that which corresponds to the difference of depths above and below the sluice, where both are in train.

Let therefore H be the depth above the sluice, and h the depth in the canal. Let e be the elevation of the sluice above the sole, and let b be its breadth. The discharge will be $e\,b\sqrt{\mathrm{H}-h}\,\sqrt{2\mathrm{G}}$ for the sluice, and $w\,h\,\frac{\sqrt{\mathrm{N}g}}{\sqrt{s}}\sqrt{\frac{w\,h}{+\,2\,h}}$ for the canal. These must be the same.

This gives the equation $e\,b\,\sqrt{H-h}\,\sqrt{2G} = w\,h\,\frac{\sqrt{Ng}}{\sqrt{s}}\sqrt{\frac{w\,h}{w+2\,h}}$ containing the solution of all the questions which can be proposed. The only uncertainty is in the quantity G, which expresses the velocity competent to the passage of the water through the orifice, circumstanced as it is, namely, subjected to contraction. This may be regulated by a proper form given to the entry into this orifice. The contraction may be almost annihilated by making the masonry of a cyclodial form on both sides, and also at the lower edge of the sluice-board, so as to give the orifice a form resembling Fig. 5, D, in the article RIVERS. If the sluice is thin in the face of a basin, the contraction will reduce 2 G to 296. If the sluice be as wide as the canal, 2 G will be nearly 500.

Question 4. Given the head of water in the basin H, the breadth b, and elevation e of the sluice, and the breadth w and slope s of the canal, to find the depth h of the stream, the velocity, and the discharge?

We must (as in *Question* 2.) make a first supposition for h, in order to find the proper value of d. Then the equation $e\,b\,\sqrt{H-h}\,\sqrt{2G} = w\,h\,\frac{\sqrt{Ng}}{\sqrt{s}}$ gives $h = -\frac{G\,e^2\,b^2\,s}{w^2\,N\,g\,d} + \sqrt{\frac{G\,e^2\,b^2\,s\,H}{w^2\,N\,g\,d} + \left(\frac{G\,e^2\,b^2\,s}{w^2\,N\,g\,d}\right)^2}$. If this value shall differ considerably from the one which we assumed in order to begin the computation, make use of it for obtaining a new value of d, and repeat the operation. We shall rarely be obliged to perform a third operation.

The following is of frequent use:

Question 5. Given the dimensions and the slope, with the velocity and discharge of a river in its ordinary state, required the area or section of the sluice which will raise the waters to a certain height, still allowing the same quantity of water to pass through? Such an operation may render the river navigable for small craft or rafts above the sluice.

The problem is reduced to the determination of the size of orifice which will discharge this water with a velocity competent to the height to which the river is to be raised; only we must take into consideration the velocity of the water above the sluice, considering it as produced by a fall which makes a part of the height productive of the whole velocity at the sluice. Therefore H, in our investigation, must consist of the height to which we mean to raise the waters, and the height which will produce the velocity with which the waters approach the sluice: h, or the depth of the stream, is the ordinary depth of the river. Then (using the former symbols) we have $e\,b\,\frac{w\,h\,\sqrt{N\,g\,d},}{\sqrt{2\,G\,s\,(H - h)}}, = \frac{Q}{\sqrt{2\,G\,(H - h)}}$.

If the area of the sluice is known, and we would learn the height to which it will raise the river, we have $H - h = \frac{Q^2}{2\,G\,e^2\,b^2}$ for the expression of the rise of the water above its ordinary level. But from this we must take the height which would produce the velocity of the river; so that if the sluice were as wide as the river, and were raised to the ordinary surface of the water, $\frac{Q^2}{2\,G\,e^2\,b^2}$, which expresses the height that produces the velocity under the sluice, must be equal to the depth of the river, and $H - h$ will be $= 0$.

The performance of aqueduct drains is a very important thing, and merits our attention in this place. While the art of managing waters, and of conducting them so as to answer our demands, renders us very important service by embellishing our habitations, or promoting our commercial intercourse, the art of draining creates as it were new riches, fertilising tracts of bog or marsh, which was not only useless, but hurtful by its unwholesome exhalations, and converting them into rich pastures and gay meadows. A wild country, occupied by marshes which are inaccessible to herds or flocks, and serve only for the haunts of water-fowls, or the retreat of a few poor fishermen, when once it is freed from the waters in which it is drowned, opens its lap to receive the most precious seeds, is soon clothed in the richest garb, gives life and abundance to numerous herds, and never fails to become the delight of the industrious cultivator who has enfranchised it, and is attached to it by the labour which it cost him. In return, it procures him abundance, and supplies him with the means of daily augmenting its fertility. No species of agriculture exhibits such long-continued and progressive improvement. New families flock to the spot, and there multiply; and there nature seems the more eager to repay their labours, in proportion as she has been obliged, against her will, to keep her treasures locked up for a longer time, chilled by the waters. The countries newly inhabited by the human race, as is a great part of America, especially to the southward, are still covered to a great extent with marshes and lakes; and they would long remain in this condition, if population, daily making new advances, did not increase industry, by multiplying the cultivating hands, at the same time that it increases their wants. The Author of this beautiful world has at the beginning formed the great masses of mountains, has scooped out the dales and sloping hills, has traced out the courses, and even formed the beds of the rivers: but he has left to man the care of making his place of abode, and the field which must feed him, dry and comfortable. For this task is not beyond his powers, as the others are. Nay, by having this given to him in charge, he is richly repaid for his labour by the very state in which he finds those countries into which he penetrates for the first time. Being covered with lakes and forests, the juices of the soil are kept for him as it were in reserve.

The air, the burning heat of the sun, and the continual washing of rains, would have combined to expend and dissipate their vegetable powers, had the fields been exposed in the same degree to their action as in the inhabited and cultivated countries, the most fertile moulds of which are long since lodged in the bottom of the ocean. All this would have been completely lost through the whole extent of South America, had it not been protected by the forests which man must cut down, by the rank herbage which he must burn, and by the marsh and bog which he must destroy by draining. Let not ungrateful man complain of this. It is his duty to take on himself the task of opening up treasures, preserved on purpose for him with so much judgment and care. If he has discernment and sensibility, he will even thank the Author of all good, who has thus husbanded them for his use. He will co-operate with his beneficent views, and will be careful not to proceed by wantonly snatching at present and partial good, and by picking out what is most easily got at, regardless of him who is to come afterwards to uncover and extract the remaining riches of the ground. A wise administration of such a country will think it their duty to leave a just share of this inheritance to their descendants, who are entitled to expect it as the last legatees. National plans of cultivation should be formed on this principle, that the steps taken by the present cultivators for realizing part of the riches of the infant country shall not obstruct the works which will afterwards be necessary for also obtaining the remainder. This is carefully attended to in Holland and in China. No man is allowed to conduct the drains, by which he recovers a piece of marsh, in such a way as to render it much more difficult for a neighbour, or even for his own successor, to drain another piece, although it may at present be quite inaccessible. There remains in the middle of the most cultivated countries many marshes, which industry has not yet attempted to drain, and where the legislature has not been at pains to prevent many little abuses which have produced elevations in the beds of rivers, and rendered the complete draining of some spots impossible. Administration should attend to such things, because their consequences are great. The sciences and arts, by which alone these difficult and costly jobs can be performed, should be protected, encouraged, and cherished. It is only from science that we can obtain principles to direct these arts. The problem of draining canals is one of the most important, and yet has hardly ever occupied the attention of the hydraulic speculatist. We apprehend that Mr Buat's theory will throw great light on it; and regret that the very limited condition of our present work will hardly afford room for a slight sketch of what may be done on the subject. We shall, however, attempt it by a general problem, which will involve most of the chief circumstances which occur in works of that kind.

Quest. 6. Let the hollow ground A (Fig. 2.) be inundated by rains or springs, and have no outlet but the canal AB, by which it discharges its water into the neighbouring river BCDE, and that its surface is nearly on a level with that of the river at B. It can only drain when the river sinks in the droughts of summer; and even if it could then drain completely, the putrid marsh would only be an infecting neighbour. It may be proposed to drain it by one or more canals; and it is required to determine their lengths and other dimensions, so as to produce the best effects?

It is evident that there are many circumstances to determine the choice, and many conditions to be attended to.

If the canals AC, AD, AE, are respectively equal to the portions BC, BD, BE, of the river, and have the same slopes, they will have the same discharge: but they are not for this reason equivalent. The long canal AE may drain the marsh completely, while the short one AC will only do it in part; because the difference of level between A and C is but inconsiderable. Also the freshes of the river may totally obstruct the operation of AC, while the canal AE cannot be hurt by them, E being so much lower than C. Therefore the canal must be carried so far down the river, that no freshes there shall ever raise the waters in the canal so high as to reduce the slope in the upper part of it to such a level that the current shall not be sufficient to carry off the ordinary produce of water in the marsh.

Still the problem is indeterminate, admitting many solutions. This requisite discharge may be accomplished by a short but wide canal, or by a longer and narrower. Let us first see what solution can be made, so as to accomplish our purpose in the most economical manner, that is, by means of the smallest equation.—We shall give the solution in the form of an example.

Suppose that the daily produce of rains and springs raises the water $1\frac{1}{2}$ inch on an area of a square league, which gives about 120,000 cubic fathoms of water. Let the bottom of the basin be three feet below the surface of the freshes in the river at B in winter. Also, that the slope of the river is 2 inches in 100 fathoms, or $\frac{1}{3600}$dth, and that the canal is to be 6 feet deep.

The canal being supposed nearly parallel to the river, it must be at least 1800 fathoms long before it can be admitted into the river, otherwise the bottom of the bog will be lower than the mouth of the canal; and even then a hundred or two more fathoms added to this will give it so little slope, that an immense breadth will be necessary to make the discharge with so small a velocity. On the other hand, if the slope of the canal be made nearly equal to that of the river, an extravagant length will be necessary before its admission into the river, and many obstacles may then intervene. And even then it must have a breadth of 13 feet, as may easily be calculated by the general hydraulic theorem. By receding from each of these extremes, we shall diminish the expense of excavation. Therefore, Let x and y be the breadth and length, and h the depth (6 feet) of the canal. Let q be the depth of the bog below the surface of the river, opposite to the basin, D the discharge in a second, and $\frac{1}{a}$ the slope of the river. We must make $h\,x\,y$ a minimum, or $x\dot{y} + y\dot{x} = o$.

The general formula gives the velocity

$$V = \frac{\sqrt{ng}\,(\sqrt{d} - 0{,}1)}{\sqrt{s} - \mathrm{L}\sqrt{s+1{,}6}} - 0{,}3\,(\sqrt{d} - 0{,}1).$$

This would give x and y; but the logarithmic term renders it very complicated. We may make use of the simple form $V = \frac{\sqrt{Ngd}}{\sqrt{S}}$, making $\sqrt{Ng}$ nearly $2\,y\,b$. This will be sufficiently exact for all cases which do not deviate far from this, because the velocities are very nearly in the subduplicate ratio of the slopes.

To introduce these data into the equation, recollect that $V = \frac{D}{h\,x}$; $d = \frac{h\,x}{x+2\,h}$. As to S, recollect that the canal being supposed of nearly equal length with the river $\frac{y}{a}$ will express the whole difference of height, and $\frac{y}{a} - q$ is the difference of height for the canal. This quantity being divided by y, gives the value of $\frac{1}{S} = \frac{\frac{y}{a} - q}{y}$. Therefore the equation for the canal becomes $\sqrt{Ng}\sqrt{\frac{h\,x}{x+2\,h}}\sqrt{\frac{\frac{y}{a} - q}{y}}$. Hence we deduce $y = \frac{N\,g\,q\,h^3x^3}{\frac{Ngh^3x^3}{a} - D^2\,(x+2\,h)}$

and $\dot{y} = \frac{3\,N\,g\,q\,h^3x^2\dot{x}}{\frac{N\,g\,h^3x^3}{a} - D^2(x+2\,h)}$

$$-\frac{N g q h^3 x^3 \dot{x}\left(\frac{3 N g h^3 x^2}{a} - D^2\right)}{\left(\frac{N g h^3 x^3}{a} - D^2 (x+2h)\right)^2}.$$ If we substitute these values in the equation $y \dot{x} + x \dot{y} = 0$, and reduce it, we obtain finally,

$$\frac{N g h^3 x^3}{a D^2} - 8 x = 8 h.$$

If we resolve this equation by making $N g = (296)^2$, or 87616 inches; $h = 72$, $\frac{1}{a} = \frac{1}{3600}$, and $D = 518400$, we obtain $x = 392$ inches, or 32 feet 8 inches, and $\frac{D}{h x}$ or $V = 18{,}36$ inches. Now, putting these values in the exact formula for the velocity, we obtain the slope of the canal, which is $\frac{1}{11664}$, nearly 0,62 inches in 100 fathoms.

Let l be the length of the canal in fathoms. As the river has 2 inches fall in 100 fathoms, the whole fall is $\frac{2l}{100}$ and that of the canal is $\frac{0{,}62l}{100}$. The difference of these two must be 3 feet, which is the difference between the river and the entry of the canal. We have therefore $\left(\frac{2-0{,}62}{100}\right)l = 36$ inches. Hence $l = 2604$ fathoms; and this multiplied by the section of the canal gives 14177 cubic fathoms of earth to be removed.

This may surely be done, in most cases, for eight shillings each cubic fathom, which does not amount to L.6000, a very moderate sum for completely draining of nine square miles of country.

In order to judge of the importance of this problem, we have added two other canals, one longer and the other shorter, having their widths and slopes so adjusted as to ensure the same performance.

Width. *Feet.*	Velocity. *Inches.*	Slope.	Length.	Excavation.
42	14,28	$\frac{1}{18788}$	2221	15547
$32\frac{2}{3}$	18,36	$\frac{1}{11664}$	2604	14177
21	28,57	$\frac{1}{4761}$	7381	25833

We have considered this important problem in its most simple state. If the basin is far from the river, so that the drains are not nearly parallel to it, and therefore have less slope attainable in their course, it is more difficult. Perhaps the best method is to try two very extreme cases and a middle one, and then a fourth, nearer to that extreme which differs least from the middle one in the quantity of excavation. This will point out on which side the minimum of excavation lies, and also the law by which it diminishes and afterwards increases. Then draw a line, on which set off from one end the lengths of the canals. At each length erect an ordinate representing the excavation; and draw a regular curve through the extremities of the ordinates. From that point of the curve which is nearest to the base line, draw another ordinate to the base. This will point out the best length of the canal with sufficient accuracy. The length will determine the slope, and this will give the width, by means of the general theorem. N. B.—These draining canals must always come off from the basin with evasated entries. This will prevent the loss of much fall at the entry.

Two canals may sometimes be necessary. In this case expense may frequently be saved, by making one canal flow into the other. This, however, must be at such a distance from the basin, that the swell produced in the other by this addition may not reach back to the immediate neighbourhood of the basin, otherwise it would impede the performance of both. For this purpose, recourse must be had to the problem III. in the article River. We must here observe, that in this respect canals differ exceedingly from rivers: rivers enlarge their beds, so as always to convey every increase of waters; but a canal may be gorged through its whole length, and will then greatly diminish its discharge. In order that the lower extremity of a canal may convey the waters of an equal canal admitted into it, their junction must be so far from the basin, that the swell occasioned by raising its waters nearly ½ more (viz. in the subduplicate ratio of 1 to 2) may not reach back to the basin.

This observation points out another method of economy. Instead of one wide canal, we may make a narrower one of the whole length, and another narrow one reaching part of the way, and communicating with the long canal at a proper distance from the basin. But the lower extremity will now be too shallow to convey the waters of both. Therefore raise its banks by using the earth taken from its bed, which must at any rate be disposed of. Thus the waters will be conveyed, and the expense, even of the lower part of the long canal, will scarcely be increased.

These observations must suffice for an account of the management of open canals; and we proceed to the consideration of the conduct of water in pipes.

This is much more simple and regular, and the general theorem requires very trifling modifications for adapting it to the cases or questions that occur in the practice of the civil engineer. Pipes are always made round, and therefore d is always ¼th of the diameter. The velocity of water in a pipe which is in train, is $= V, = \frac{307 (\surd d - 0,1)}{\surd s - L \surd \overline{s + 1,6}} - 0,3 (\surd d - 0,1)$ or $= (\surd d - 0,1) \left(\frac{307}{\surd s - L \surd \overline{s + 1,6}} - 0,3\right)$.

The chief questions are the following:

Quest. 1. Given the height H of the reservoir above the place of delivery, and the diameter and length of the pipe, to find the quantity of water discharged in a second?

Let L be the length, and h the fall which would produce the velocity with which the water enters the pipe, and actually flows in it, after overcoming all obstructions. This may be expressed in terms of the velocity by $\frac{V^2}{2G}$, G denoting the acceleration of gravity, corresponding to the manner of entry. When no methods are adopted for facilitating the entry of the water, by a bell-shaped funnel or otherwise, 2 G may be assumed as = 500 inches, or 42 feet, according as we measure the velocity in inches or feet. The slope is $\frac{1}{s}, = \frac{H - \frac{V^2}{2G}}{L}$, which must be put into the general formula. This would make it very complicated. We may simplify it by the consideration that the velocity is very small in comparison of that arising from the height H: consequently h is very small. Also, in the same pipe, the resistances are nearly in the duplicate ratio of the velocities when these are small, and when they differ *little* among themselves. Therefore make $b = \frac{L}{h}$, taking h by guess, a very little less than H. Then compute the mean velocity v corresponding to these data, or take it from the table. If $h + \frac{v^2}{2G}$ be $=$ H, we have found the mean velocity $V = v$. If not, make the following proportion:

$$h : \frac{v^2}{2G} = H - \frac{V^2}{2G} : \frac{V^2}{2G},$$

which is the same with this

$$h + \frac{v^2}{2G} : v^2 = H : V^2,$$

and V^2 is $= \frac{v^2 H}{h + \frac{v^2}{2G}}, = \frac{v^2 H}{\frac{2Gh + v^2}{2G}} = \frac{v^2 \cdot 2GH}{v^2 + 2Gh}$.

If the pipe has any bendings, they must be calculated for in the manner mentioned in the article RIVER; and the head of water necessary for overcoming this additional resistance being called $\frac{V^2}{m}$ the last proportion must be changed for

$$h+v^2\left(\frac{1}{2G}+\frac{1}{m}\right):v^2=H:V^2.$$

Quest. 2. Given the height of the reservoir, the length of the pipe, and the quantity of water which is to be drawn off in a second, to find the diameter of the pipe which will draw it off?

Let d be considered as $=\frac{1}{4}$ of the diameter, and let $1:c$ represent the ratio of the diameter of a circle to its circumference. The section of the pipe is $4\,c\,d^2$. Let the quantity of water per second be Q; then $\frac{Q}{4\,c\,d^2}$ is the mean velocity. Divide the length of the pipe by the height of the reservoir above the place of delivery, diminished by a very small quantity, and call the quotient S. Consider this as the slope of the conduit; the general formula now becomes

$$\frac{Q}{4\,d^2}=\frac{307\,(\sqrt{d}-0{,}1)}{\sqrt{s}-\mathrm{L}\sqrt{s+1{,}06}}-0{,}3\,(\sqrt{d}-0{,}1), \text{ or}$$

$$\frac{Q}{4\,c\,d^2}=\frac{(307(\sqrt{d}-0{,}1)}{\sqrt{S}}-0{,}3\,(\sqrt{d}-0{,}1).$$

We may neglect the last term in every case of civil practice, and also the small quantity 0,1. This gives the very simple formula

$$\frac{Q}{4\,c\,d^2}=\frac{307\sqrt{d}}{\sqrt{S}}$$

from which we readily deduce

$$d=\left|\overline{\frac{Q\sqrt{S}}{4\,c\times 307}}\right|^{\frac{2}{5}}=\left|\overline{\frac{Q\sqrt{S}}{3858}}\right|^{\frac{2}{5}}$$

This process gives the diameter somewhat too small. But we easily rectify this error by computing the quantity delivered by the pipe, which will differ a little from the quantity proposed. Then observing, by this equation, that two pipes having the same length and the same slope give quantities of water, of which the squares are nearly as the 5th powers of the diameter, we form a new diameter in this proportion, which will be almost perfectly exact.

It may be observed that the height assumed for determining the slope in these two questions will seldom differ more than an inch or two from the whole height of the reservoir above the place of delivery; for in conduits of a few hundred feet long the velocity seldom exceeds four feet per second, which requires only a head of 3 inches.

As no inconvenience worth minding results from making the pipes a tenth of an inch or so wider than is barely sufficient, and as this generally is more than the error arising from even a very erroneous assumption of *h*, the answer first obtained may be augmented by one or two tenths of an inch, and then we may be confident that our conduit will draw off the intended quantity of water.

We presume that every person who assumes the name of engineer knows how to reduce the quantity of water measured in gallons, pints, or other denominations, to cubic inches, and can calculate the gallons, &c. furnished by a pipe of known diameter, moving with a velocity that is measured in inches per second. We further suppose that all care is taken in the construction of the conduit, to avoid obstructions occasioned by lumps of solder hanging in the inside of the pipes; and, particularly, that all the cocks and plugs by the way have waterways equal to the section of the pipe. Undertakers are most tempted to fail here, by making the cocks too small, because large cocks are very costly. But the employer should be scrupulously attentive to this; because a simple contraction of this kind may be the throwing away of many hundred pounds in a wide pipe, which yields no more water than can pass through the small cock.

The chief obstructions arise from the deposition of sand or mud in the lower parts of pipes, or the collection of air in the upper parts of their bendings. The velocity being always very moderate, such depositions of heavy matters are unavoidable. The utmost care should therefore be taken to have the water freed from all such things at its entry by proper filtration; and there ought to be cleansing plugs at the lower parts of the bendings, or rather a very little way beyond them. When these are opened, the water issues with greater velocity, and carries the depositions with it.

It is much more difficult to get rid of the air which chokes the pipes by lodging in their upper parts. This is sometimes taken in along with the water at the reservoir, when the entry of the pipe is too near the surface. This should be carefully avoided, and it costs no trouble to do so. If the entry of the pipe is two feet under the surface, no air can ever get in. Floats should be placed above the entries, having lids hanging from them, which will shut the pipe before the water runs too low.

But air is also disengaged from spring-water by merely passing along the pipe. When pipes are supplied by an engine, air is very often drawn in by the pumps in a disengaged state. It is also disengaged from its state of chemical union, when the pumps have a suction-pipe of 10 or 12 feet, which is very common. In whatever way it is introduced, it collects in all the upper part of bendings, and chokes the passage, so that sometimes not a drop of water is delivered. Our cocks should be placed there, which should be opened frequently by persons who have this in charge. Desaguliers describes a contrivance to be placed on all such eminences, which does this of itself. It is a pipe with a cock, terminating in a small cistern. The key of the cock has a hollow ball of copper at the end of a lever. When there is no air in the main pipe, water comes out by this discharger, fills the cistern, raises the ball, and thus shuts the cock. But when the bend of the main contains air, it rises into the cistern, and occupies the upper part of it. Thus the floating ball falls down, the cock opens and lets out the air, and the cistern again filling with water, the ball rises, and the cock is again shut.

A very neat contrivance for this purpose was invented by the late Professor Russell of Edinburgh. The cylindrical pipe BCDE (Fig. 3.), at the upper part of a bending of the main, is screwed on, the upper end of which is a flat plate perforated with a small hole F. This pipe contains a hollow copper cylinder G, to the upper part of which is fastened a piece of soft leather H. When there is air in the pipe, it comes out by the hole A, and occupies the discharger, and then escapes through the hole F. The water follows, and, rising in the discharger, lifts up the hollow cylinder G, causing the leather H to apply itself to the plate CD, and shut the hole. Thus the air is discharged without the smallest loss of water.

It is of the most material consequence that there be no contraction in any part of a conduit. This is evident; but it is also prudent to avoid all unnecessary enlargements. For when the conduit is full of water moving along it, the velocity in every section is inversely proportional to the area of the section: it is therefore diminished wherever the pipe is enlarged; but it must again be increased where the pipe contracts. This cannot be without expending force in the acceleration. This consumes part of the impelling power, whether this be a head of water, or the force of an engine. See what is said on this subject in the following dissertation on Pumps. Nothing is gained by any enlargement; and every contraction, by requiring an augmentation of velocity, employs a part of the impelling force precisely equal to the weight of a column of water whose base is the contracted passage, and whose height is the fall which would produce a velocity equal to this augmentation. This point seems to have been quite overlooked by engineers of the first eminence, and has in many instances greatly diminished the performance of their best works. It is no less detrimental in open canals; because at every contraction a small fall is required for restoring the velocity lost in the enlargement of the canal, by which the general slope and velocity are diminished. Another point which must be attended to in the conducting of water is, that the motion should not be subsultory, but continuous. When water is to be driven along a main by the strokes of a reciprocating engine, it should be forced into an air-box, the spring of which may preserve it in motion along the whole subsequent main. If the water is brought to rest at every successive stroke of the piston, the whole mass must again be put in motion through the whole length of the main. This requires the same useless expenditure of power as to communicate this motion to as much dead matter; and this is over and above the force which may be necessary for raising the water to a certain height; which is the only circumstance that enters into the calculation of the power of the pump-engine.

An air-box removes this imperfection, because it keeps up the motion during the returning stroke of the piston. The compression of the air by the active stroke of the piston must be such as to continue the impulse in opposition to the contrary pressure of the water (if it is to be raised to some height), and in opposition to the friction or other resistances which arise from the motion that the water really acquires. Indeed a very considerable force is employed here also in changing the motion of the water, which is forced out of the capacious air-box into the narrow pipe; and when this change of motion is not judiciously managed, the expenditure of power may be as great as if all were brought to rest and again put into motion. It may even be greater, by causing the water to move in the opposite direction to its former motion. Of such consequence is it to have all these circumstances scientifically considered. It is in such particulars, unheeded by the ordinary herd of engineers or pump-makers, that the superiority of an intelligent practitioner is to be seen.

Another material point in the conduit of water in pipes is the distribution of it to the different persons who have occasion for it. This is rarely done from the rising main. It is usual to send the whole into a cistern, from which it is afterwards conducted to different places in separate pipes. Till the discovery of the general theorem by the Chevalier Buat, this has been done with great inaccuracy. Engineers think that the different purchasers from water-works receive in proportion to their respective bargains when they give them pipes whose areas are proportional to these payments. But we now see, that when these pipes are of any considerable length, the waters of a larger pipe run with a greater velocity than those of a smaller pipe having the same slope. A pipe of two inches diameter will give much more water than four pipes of one inch diameter; it will give as much as five and a half such pipes, or more; because the squares of the discharges are very nearly as the fifth powers of the diameters. This point ought therefore to be carefully considered in the bargains made with the proprietors of water-works, and the payments made in this proportion. Perhaps the most unexceptionable method would be to make a double distribution. Let the water be first let off in its proper proportions into a second series of small cisterns, and let each have a pipe which will convey the whole water that is discharged into it. The first distribution may be made entirely by pipes of one inch in diameter; this would leave nothing to the calculation of the distributor, for every man would pay in proportion to the number of such pipes which run into his own cistern.

In many cases, however, water is distributed by pipes derived from a main. And here another circumstance comes into action. When water is passing along a pipe, its pressure on the sides of the pipe is diminished by its velocity; and if a pipe is now derived from it, the quantity drawn off is also diminished in the subduplicate ratio of the pressures. If the pressure is reduced to $\frac{1}{4}$th, $\frac{1}{9}$th,

$\frac{1}{16}$th, &c. the discharge from the lateral pipe is reduced to $\frac{1}{2}$, $\frac{1}{3}$d, $\frac{1}{4}$th, &c.

It is therefore of great importance to determine, what this diminution of pressure is which arises from the motion along the main.

It is plain, that if the water suffered no resistance in the main, its velocity would be that with which it entered, and it would pass along without exerting any pressure. If the pipes were shut at the end, the pressure on the sides would be the full pressure of the head of water. If the head of water remain the same, and the end of the tube be contracted, but not stopped entirely, the velocity in the pipe is diminished. If we would have the velocity in the pipe with this contracted mouth augmented to what it was before the contraction was made, we must employ the pressure of a piston, or of a head of water. This is propagated through the fluid, and thus a pressure is immediately excited on the sides of the pipe. New obstructions of any kind, arising from friction or any other cause, produce a diminution of velocity in the pipe. But when the natural velocity is checked, the particles react on what obstructs their motion; and this action is uniformly propagated through a perfect fluid in every direction. The resistance therefore which we thus ascribe to friction, produces the same lateral pressure which a contraction of the orifice, which equally diminishes the velocity in the pipe, would do. Indeed this is demonstrable from any distinct notions that we can form of these obstructions. They proceed from the want of perfect smoothness, which obliges the particle next the sides to move in undulated lines. This excites transverse forces in the same manner as any constrained curvilineal motion. A particle in its undulated path tends to escape from it, and acts on the lateral particles in the same manner that it would do if moving simply in a capillary tube having the same undulations; it would press on the concave side of every such undulation. Thus a pressure is exerted among the particles, which is propagated to the sides of the pipe; or the diminution of velocity may arise from a viscidity or want of perfect fluidity. This obliges the particle immediately pressed to drag along with it another particle which is withheld by adhesion to the sides. This requires additional pressure from a piston, or an additional head of water; and this pressure also is propagated to the sides of the pipe.

Hence it should follow, that the pressure which water in motion exerts on the sides of its conduit is equal to that which is competent to the head of water which impels it into the pipe, diminished by the head of water competent to the actual velocity with which it moves along the pipe. Let H represent the head of water which impels it into the entry of the pipe, and h the head which would produce the actual velocity; then $H-h$ is the column which would produce the pressure exerted on its sides.

This is abundantly verified by very simple experiments. Let an upright pipe be inserted into the side of the main pipe. When the water runs out by the mouth of the main, it will rise in this branch till the weight of the column balances the pressure that supports it; and if we then ascertain the velocity of the issuing water by means of the quantity discharged, and compute the head or height necessary for producing this velocity, and subtract this from the height of water above the entry of the main, we shall find the height in the branch precisely equal to their difference. Our readers may see this by examining the experiments related by Gravesande, and still better by consulting the experiments narrated by Bossut, § 558, which are detailed with great minuteness; the results corresponded accurately with this proposition. The experiments indeed were not heights of water supported by this pressure, but water expelled by it through the same orifice. Indeed the truth of the proposition appears in every way we can consider the motion of water. And as it is of the first importance in the practice of conducting water (for reasons which will presently appear), it merits a particular attention. When an inclined tube is in train, the accelerating power of the water (or its weight diminished in the proportion of the length of the oblique column to its vertical height, or its weight multiplied by the fraction $\frac{1}{s}$, which expresses the slope), is in equilibrio with the obstructions; and therefore it exerts no pressure on the pipe but what arises from its weight alone. Any part of it would continue to slide down the inclined plane with a constant velocity, though detached from what follows it. It therefore derives no pressure from the head of water which impelled it into the pipe. The same must be said of a horizontal pipe infinitely smooth, or opposing no resistance. The water would move in this pipe with the full velocity due to the head of water which impels it into the entry. But when the pipe opposes an obstruction, the head of water is greater than that which would impel it into the pipe with the velocity that it actually has in it; and this additional pressure is propagated along the pipe, where it is balanced by the actual resistance, and therefore excites a *quaqua versum* pressure on the pipe. In short, whatever part of the head of water in the reservoir, or of the pressure which impels it along the tube, is not employed in producing velocity, is employed in acting against some obstruction, and excites (by the reaction of this obstruction) an equal pressure on the tube. The rule therefore is general, but is subject to some modifications which deserve our attention.

In the simply inclined pipe BC (Fig. 4.), the pressure on any point S is equal to that of the head AB of water which impels the water into the pipe wanting, or *minus* that of the head of water which would communicate to it the velocity with which it actually moves. This we shall call x, and consider it as the weight of a column of water ately within this is withheld by the outer film, but glides through it: and thus all the concentric films glide within those around them, somewhat like the sliding tubes of a spy-glass, when we draw it out by taking hold of the end of the innermost. Thus the second film passes beyond the first or outermost, and becomes the outermost and rubs along the tube. The third does the same in its turn; and thus the central filaments come at last to the outside, and all sustain their greatest possible obstruction. When this is accomplished, the pipe is in train. This requires a certain length, which we cannot determine by theory. We see, however, that pipes of greater diameter must require a greater length, and this in a proportion which is probably that of the number of filaments, or the square of the diameter. Buat found this supposition agree well enough with his experiments. A pipe of one inch in diameter sustained no change of velocity by gradually shortening it till he reduced it to six feet, and then it discharged a little more water. A pipe of two inches diameter gave a sensible augmentation of velocity when shortened to 25 feet. He therefore says, that the square of the diameter in inches, multiplied by 72, will express (in inches) the length necessary for putting any pipe in train.

The resistance exerted by a square inch of the pipe makes but a small part of the pressure which the whole resistances occasion to be exerted there before they can be overcome. The resistance may be represented by $\frac{d}{s}$, when d is the hydraulic depth ($\frac{1}{4}$th of the diameter), and s the length of a column whose vertical height is one inch, and it is the relative weight of a column of water whose base is a square inch, and height is d. For the resistance of any length s of pipe which is in train, is equal to the tendency of the water to slide down (being balanced by it); that is, equal to the weight of this column multiplied by $\frac{1}{s}$. The magnitude of this column is had by multiplying its length by its section. The section is the product of the border b or circumference, multiplied by the mean depth d, or it is $b\,d$. This, multiplied by the length, is $b\,d\,s$; and this multiplied by the slope $\frac{1}{s}$ is $b\,d$, the relative weight of the column whose length is s. The relative weight of one inch is therefore $\frac{bd}{s}$; and this is in equilibrio with the resistance of a ring of the pipe one inch broad. This, when unfolded, is a parallelogram b inches in length. One inch of this therefore is $\frac{d}{s}$, the relative weight of a column of water having d for its height and a square inch for its base. Suppose the pipe four inches in diameter, and the slope $= 253$, the resistance is one grain; for an inch of water weighs 253 grains.

This knowledge of the pressure of water in motion is of great importance. In the management of rivers and canals it instructs us concerning the damages which they produce in their beds by tearing up the soil; it informs us of the strength which we must give to the banks: but it is of more consequence in the management of close conduits. By this we must regulate the strength of our pipes; by this also we must ascertain the quantities of water which may be drawn off by lateral branches from any main conduit.

With respect to the first of these objects, where security is our sole concern, it is proper to consider the pressure in the most unfavourable circumstances, viz. when the end of the main is shut. This case is not unfrequent. Nay, when the water is in motion, its velocity in a conduit seldom exceeds a very few feet in a second. Eight feet *per* second requires only one foot of water to produce it. We should therefore estimate the strain in all conduits by the whole height of the reservoir.

In order to adjust the strength of a pipe to the strain, we may conceive it as consisting of two half cylinders of insuperable strength, joined along the two seams, where the strength is the same with the ordinary strength of the materials of which it is made. The inside pressure tends to burst the pipe by tearing open these seams, and each of them sustains half of the strain. The strain on an inch of these two seams is equal to the weight of a column of water whose height is the depth of the seam below the surface of the reservoir, and whose base is an inch broad and a diameter of the pipe in length. This follows from the common principles of hydrostatics.

Suppose the pipe to be of lead, one foot in diameter and 100 feet under the surface of the reservoir. Water weighs $62\frac{1}{2}$ pounds *per* foot. The base of our column is therefore $\frac{1}{12}$th of a foot, and the tendency to burst the pipe is $100 \times 62\frac{1}{2} \times \frac{1}{12} = \frac{6250}{12}, = 521$ pounds nearly. Therefore an inch of one seam is strained by $260\frac{1}{2}$ pounds. A rod of lead one inch square is pulled asunder by 860 pounds (STRENGTH OF MATERIALS, vol. I.). Therefore, if the thickness of the seam is $= \frac{260}{860}$ inches, or $\frac{1}{3}$d of an inch, it will just withstand this strain. But we must make it much stronger than this, especially if the pipe leads from an engine which sends the water along it by starts. Belidor and Desaguiliers have given tables of the thickness and weights of pipes which experience has found sufficient for the different materials and depths. Desaguiliers says, that a leaden pipe of $\frac{3}{4}$ths of an inch in thickness is strong enough for a height of 140 feet and diameter of 7 inches. From this we may calculate others. Belidor says, that a leaden pipe 12 inches diameter and 60 feet deep should be half an inch thick: but these things will be more properly computed by means of the list given in the article STRENGTH OF MATERIALS, vol. I.

The application which we are most anxious to make of the knowledge of the pressure of moving waters is the derivation from a main conduit by lateral branches. This occurs very frequently in the distribution of waters among the inhabitants of towns; and it is so imperfectly understood by the greatest part of those who take the name of engineers, that individuals have no security that they shall get even one half of the water they bargain and pay for; yet this may be as accurately ascertained as any other problem in hydraulics by means of our general theorem. The case therefore merits our particular attention.

It appears to be determined already, when we have ascertained the pressures by which the water is impelled into these lateral pipes, especially after we have said that the experiments of Bossut on the actual discharges from a lateral pipe fully confirm the theoretical doctrine. But much remains to be considered. We have seen that there is a vast difference between the discharge made through a hole, or even through a short pipe, and the discharge from the far end of a pipe derived from a main conduit. And even when this has has been ascertained by our new theory, the discharge thus modified will be found considerably different from the real state of things: for when water is flowing along a main with a known velocity, and therefore exerting a known pressure on the circle which we propose for the entry of a branch, if we insert a branch there water will go along it: but this will generally make a considerable change in the motion along the main, and therefore in the pressure which is to expel the water. It also makes a considerable change in the whole quantity which passes along the anterior part of the main, and a still greater change on what moves along that part of it which lies beyond the branch: it therefore affects the quantity necessary for the whole supply, the force that is required for propelling it, and the quantity delivered by other branches. This part therefore of the management of water in conduits is of considerable importance and intricacy. We can propose in this place nothing more than a solution of such leading questions as involve the chief circumstances, recommending to our readers the perusal of original works on this subject. M. Bossut's experiments are fully competent to the establishment of the fundamental principle. The hole through which the lateral discharges were made was but a few feet from the reservoir. The pipe was successively lengthened, by which the resistances were increased, and the velocity diminished. But this did not affect the lateral discharges, except by affecting the pressures; and the discharges from the end of the main were supposed to be the same as when the lateral pipe was not inserted. Although this was not strictly true, the difference was insensible, because the lateral pipe had but about the 18th part of the area of the main.

Suppose that the discharge from the reservoir remains the same after the derivation of this branch, then the motion of the water all the way to the insertion of the branch is the same as before; but, beyond this, the discharge is diminished by all that is discharged by the branch, with the head x equivalent to the pressure on the side. The discharge by the lower end of the main being diminished, the velocity and resistance in it are also diminished. Therefore the difference between x and the head employed to overcome the friction in this second case, would be a needless or inefficient part of the whole load at the entry, which is impossible; for every force produces an effect, or it is destroyed by some reaction. The effect of the forcing head of water is to produce the greatest discharge corresponding to the obstructions; and thus the discharge from the reservoir, or the supply to the main, must be augmented by the insertion of the branch, if the forcing head of water remains the same. A greater portion therefore of the forcing head was employed in producing a greater discharge at the entry of the main, and the remainder, less than x, produced the pressure on the sides. This head was the one competent to the obstructions resulting from the velocity beyond the insertion of the branch; and this velocity, diminished by the discharge already made, was less than that at the entry, and even than that of the main without a branch. This will appear more distinctly by putting the case into the form of an equation. Therefore let $H - x$ be the height due to the velocity at the entry, of which the effect obtains only horizontally. The head x is the only one which acts on the sides of the tube, tending to produce the discharge by the branch, at the same time that it must overcome the obstructions beyond the branch. If the orifice did not exist, and if the force producing the velocity on a short tube be represented by $2G$, and the section of the main by A, the supply at the entry of the main would be $A\sqrt{2G}\sqrt{H-x}$; and if the orifice had no influence on the value of x, the discharge by the orifice would be $D\sqrt{\frac{x}{H}}$, D being its discharge by means of the head H, when the end of the main is shut; for the discharges are in the subduplicate ratio of the heads of water by which they are expelled; and therefore $\sqrt{H} : \sqrt{x} = D : D\sqrt{\frac{x}{H}}$ $(=\delta)$. But we have seen that x must diminish; and we know that the obstructions are nearly as the square roots of the velocities, when these do not differ much among themselves. Therefore calling y the pressure or head which balances the resistances of the main without a branch, while x is the head necessary for the main with a branch, we may institute this proportion $y : H - y = x : \frac{x(H-y)}{y}$; and this fourth term will express the head producing the velocity in the main beyond the branch (as $H - y$ would have done in a main without a branch). This velocity beyond the branch will be $\sqrt{2G}\sqrt{\frac{x(H-y)}{y}}$, and the discharge at the end will be $A\sqrt{2G}\sqrt{\frac{x(H-y)}{y}}$. If to this we add the discharge of the branch, the sum will be the whole discharge, and therefore the whole supply. Therefore we have the following equation, $A\sqrt{2G}\sqrt{H-y} =$

$A\sqrt{2G}\sqrt{\frac{x(H-y)}{y}}+D\sqrt{\frac{x}{H}}$. From this we deduce the value of $x=\frac{2GHA^2}{\left(A\sqrt{2G}\sqrt{\frac{H-y}{y}}+\frac{D}{\sqrt{H}}\right)^2+2GA^2}$. This value of x being substituted in the equation of the discharge δ of the branch, which was $=D\sqrt{\frac{x}{H}}$, will give the discharges required, and they will differ so much the more from the discharges calculated according to the simple theory, as the velocity in the main is greater. By the simple theory, we mean the supposition that the lateral discharges are such as would be produced by the head $H-h$, where H is the height of the reservoir, and h the head due to the actual velocity in the main.

And thus it appears that the proportion of the discharge by a lateral pipe from a main that is shut at the far end, and the discharge from a main that is open, depends not only on the pressures, but also on the size of the lateral pipe, and its distance from the reservoir. When it is large, it greatly alters the train of the main, under the same head, by altering the discharge at its extremity, and the velocity in it beyond the branch; and if it be near the reservoir, it greatly alters the train, because the diminished velocity takes place through a greater extent, and there is a greater diminution of the resistances.

When the branch is taken off at a considerable distance from the reservoir, the problem becomes more complicated, and the head x is resolved into two parts; one of which balances the resistance in the first part of the main, and the other balances the resistances beyond the lateral pipe, with a velocity diminished by the discharge from the branch.—A branch at the end of the main produces very little change in the train of the pipe.

When the lateral discharge is great, the train may be so altered, that the remaining part of the main will not run full, and then the branch will not yield the same quantity. The velocity in a very long horizontal tube may be so small (by a small head of water and great obstructions in a very long tube) that it will just run full. An orifice made in its upper side will yield nothing; and yet a small tube inserted into it will carry a column almost as high as the reservoir. So that we cannot judge in all cases of the pressures by the discharges, and *vice versa*.

If there be an inclined tube, having a head greater than what is competent to the velocity, we may bring it into train, by an opening on its upper side near the reservoir. This will yield some water, and the velocity will diminish in the tube till it is in train. If we should now enlarge the hole, it will yield no more water than before.

And thus we have pointed out the chief circumstances which affect these lateral discharges. The discharges are afterwards modified by the conduits in which they are conveyed to their places of destination. These being generally of small dimensions, for the sake of economy, the velocity is much diminished. But, at the same time, it approaches nearer to that which the same conduit would bring directly from the reservoir, because its small velocity will produce a less change in the train of the main conduit.

We shall now treat of jets of water, which still make an ornament in the magnificent pleasure grounds of the wealthy. Some of these are indeed grand objects, such as the two at Peterhoff in Russia, which spout about 60 feet high a column of nine inches diameter, which falls again, and shakes the ground with its blow. Even a spout of an inch or two inches diameter, lancing to the height of 150 feet, is a gay object, and greatly enlivens a pleasure-ground, especially when the changes of a gentle breeze bend the jet to one side. But we have no room left for treating this subject, which is of some nicety; and must conclude this article with a very short account of the management of water as an active power for impelling machinery.

II. *Of Machinery driven by Water.*

This is a very comprehensive article, including almost every possible species of mill. It is no less important, and it is therefore matter of regret that we cannot enter into the detail which it deserves. The mere description of the immense variety of mills which are in general use would fill volumes, and a scientific description of their principles and maxims of construction would almost form a complete body of mechanical science. But this is far beyond the limits of a work like ours. Many of these machines have been already described under their proper names, or under the articles which give an account of their manufactures; and for others we must refer our readers to the original works, where they are described in minute detail. The great academical collection *Des Arts et Metiers*, published at Paris in many folio volumes, contains a description of the peculiar machinery of many mills; and the volumes of the *Encyclopédie Methodique*, which particularly relate to the mechanic arts, already contain many more. All that we can do in this place is, to consider the chief circumstances that are common to all water-mills, and from which all must derive their efficacy. These circumstances are to be found in the manner of employing water as an acting power, and most of them are comprehended in the construction of water-wheels. When we have explained the principles and the maxims of construction of a water-wheel, every reader conversant in mechanics knows, that the axis of this wheel may be employed to transmit the force impressed on it to any species of machinery. Therefore nothing subsequent to this can with propriety be considered as *water-works*.

Water-wheels are of two kinds, distinguished by the manner in which water is made an impelling power, viz. by its weight, or by its impulse. This requires a very different form and manner of adaptation; and this forms an ostensible distinction, sufficiently obvious to give a name to each class. When water is made to act by its weight, it is delivered from the spout as high on the wheel as possible, that it may continue long to press it down: but when it is made to strike the wheel, it is delivered as low as possible, that it may have previously acquired a great velocity. And thus the wheels are said to be OVERSHOT or UNDERSHOT.

Of Overshot Wheels.

THIS is nothing but a frame of open buckets, so disposed round the rim of a wheel as to receive the water delivered from a spout; so that one side of the wheel is loaded with water, while the other is empty. The consequence must be, that the loaded side must descend. By this motion the water runs out of the lower buckets, while the empty buckets of the rising side of the wheel come under the spout in their turn, and are filled with water.

If it were possible to construct the buckets in such a manner as to remain completely filled with water till they come to the very bottom of the wheel, the pressure with which the water urges the wheel round its axis would be the same as if the extremity of the horizontal radius were continually loaded with a quantity of water sufficient to fill a square pipe, whose section is equal to that of the bucket, and whose length is the diameter of the wheel. For let the buckets BD and EF (Fig. 5.) be compared together, the arches DB and EF are equal. The mechanical energy of the water contained in the bucket EF, or the pressure with which its weight urges the wheel, is the same as if all this water were hung on that point T of the horizontal arm CF, where it is cut by the vertical or plumb-line BT. This is plain from the most elementary principles of mechanics.

Therefore the effect of the bucket BD is to that of the bucket EF at CT to CF or CB. Draw the horizontal lines PB *b b*, QD *d d*. It is plain, that if BD is taken very small, so that it may be considered as a straight line, BD : BO = CB : BP, and EF : *b d* = CF : CT, and EF × CT = *b d* × CF. Therefore if the prism of water, whose vertical section is *b b d d*, were hung on at F, its force to urge the wheel round would be the same as that of the water lying in the bucket BD. The same may be said of every bucket; and the effective pressure of the whole ring of water A*f*HKFI, in its natural situation, is the same with the pillar of water *a h h a* hung on at F. And the effect of any portion B F of this ring is the same with that of the corresponding portion *b* F*f b* of the vertical pillar. We do not take into account the small difference which arises from the depth B or F*f*, because we may suppose the circle described through the centres of gravity of the buckets. And in the farther prosecution of this subject, we shall take similar liberties, with the view of simplifying the subject, and saving time to the reader.

But such a state of the wheel is impossible. The bucket at the very top of the wheel may be completely filled with water; but when it comes into the oblique position BD, a part of the water must run over the outer edge *z*, and the bucket will only retain the quantity ZBD*z*; and if the buckets are formed by partitions directed to the axis of the wheel, the whole water must be run out by the time that they descend to the level of the axis. To prevent this many contrivances have been adopted. The wheel has been surrounded with a hoop or sweep, consisting of a circular board, which comes almost into contact with the rim of the wheel, and terminates at H, where the water is allowed to run off. But unless the work is executed with uncommon accuracy, the wheel made exactly round, and the sweep exactly fitting it, a great quantity of water escapes between them; and there is a very sensible obstruction to the motion of such a wheel, from something like friction between the water and the sweep. Frost also effectually stops the motion of such a wheel. Sweeps have therefore been generally laid aside, although there are situations where they might be used with good effect.

Mill-wrights have turned their whole attention to the giving a form to the buckets which shall enable them to retain the water along a great portion of the circumference of the wheel. It would be endless to describe all these contrivances; and we shall therefore content ourselves with one or two of the most approved. The intelligent reader will readily see that many of the circumstances which concur in producing the ultimate effect (such as the facility with which the water is received into the buckets, the place which it is to occupy during the progress of the bucket from the top to the bottom of the wheel, the readiness with which they are evacuated, or the chance that the water has of being dragged beyond the bottom of the wheel by its adhesion, &c. &c.) are such as do not admit of precise calculation or reasoning about their merits; and that this or that form can seldom be evidently demonstrated to be the very best possible. But, at the same time, he will see the general reasons of preference, and his attention will be directed to circumstances which must be attended to, in order to have a good bucketed wheel.

Fig. 6. is the outline of a wheel having 40 buckets. The ring of board contained between the concentric circles QDS and PAR, making the ends of the buckets, is called the SHROUDING, in the language of the art, and QP is called the *depth of shrouding*. The inner circle PAR is called the SOLE of the wheel, and usually consists of boards nailed to strong wooden rings of compass timber of considerable scantling, firmly united with the arms or radii. The partitions which determine the form of the buckets consist of three different planes or boards AB, BC, CD, which are variously named by different artists. We have heard them named the START or SHOULDERS, the ARM, and the WREST (probably for wrist, on account of a resemblance of the whole line to the human arm); B is also called the ELBOW. Fig. 7. represents a small portion of the same bucketing on a larger scale, that the proportions of the parts may be more distinctly seen. AG, the sole of one bucket, is made about $\frac{1}{6}$ more than the depth GH of the shrouding. The start AB is $\frac{1}{4}$ of AI. The plane BC is so inclined to AB that it would pass through H; but it is made to terminate in C, in such a manner that FC is $\frac{5}{8}$ths of GH or AI. Then CD is so placed that HD is about $\frac{1}{5}$th of IH.

By this construction, it follows that the area FABC is very nearly equal to DABC; so that the water which will fill the space FABC will all be contained in the bucket when it shall come into such a position that AD is a horizontal line; and the line AB will then make an angle of nearly 35° with the vertical, or the bucket will be 35° from the perpendicular. If the bucket descend so much lower that one half of the water runs out, the line AB will make an angle of 250°, or 24° nearly, with the vertical. Therefore the wheel, filled to the degree now mentioned, will *begin* to lose water at about $\frac{1}{8}$th of the diameter from the bottom, and *half of the water will be discharged* from the lowest bucket, about $\frac{1}{12}$th of the diameter farther down: These situations of the discharging bucket are marked at T and V in Fig. 6. Had a greater proportion of the buckets been filled with water when they were under the spout, the discharge would have begun at a greater height from the bottom, and we should lose a greater portion of the whole fall of water. The loss by the present construction is less than $\frac{1}{10}$th (supposing the water to be delivered into the wheel at the very top), and may be estimated at about $\frac{1}{11}$th; for the loss is the versed sine of the angle which the radius of the bucket makes with the verticle. The versed sine of 35° is nearly $\frac{1}{5}$th of the radius (being

0,18085), or $\frac{1}{5}$th of the diameter. It is evident, that if only $\frac{1}{2}$ of this water were supplied to each bucket as it passes the spout, it would have been retained for 10^0 more of a revolution, and the loss of fall would have been only about $\frac{1}{8}$th.

These observations serve to show, in general, that an advantage is gained by having the buckets so capacious that the quantity of water which each can receive as it passes the spout may not nearly fill it. This may be accomplished by making them of a sufficient length, that is, by making the wheel sufficiently broad between the *two* shroudings. Economy is the only objection to this practice, and it is generally very ill placed. When the work to be performed by the wheel is great, the addition of power gained by a greater breadth will soon compensate for the additional expense.

The third plane CD is not very frequent; and millwrights generally content themselves with continuing the board all the way from the elbow B to the *outer edge* of the wheel at H; and AB is generally no more than $\frac{1}{3}$d of the depth AI. But CD is a very evident improvement, causing the wheel to retain a very sensible addition to the water. Some indeed make this addition more considerable, by bringing BC more outward, so as to meet the rim of the wheel at H, for instance, and making HD coincide with the rim. But this makes the entry of the water somewhat more difficult during the very short time that the opening of the bucket passes the spout. To facilitate this as much as possible, the water should get a direction from the spout, such as will send it into the buckets in the most perfect manner. This may be obtained by delivering the water through an aperture that is divided by thin plates of board or metal, placed in the proper position, as we have represented in Fig. 6. The form of bucket last mentioned, having the wrest concentric with the rim, is unfavourable to the ready admission of the water; whereas an oblique wrest conducts the water which has missed one bucket into the next below.

The mechanical consideration of this subject also shows us, that a deep shrouding, in order to make a capacious bucket, is not a good method: it does not make the buckets retain their water any longer; and it diminishes the effective fall of water: for the water received at the top of the wheel immediately falls to the bottom of the bucket, and thus shortens the fictitious pillar of water, which we showed to be the measure of the effective or useful pressure on the wheel: and this concurs with our former reasons for recommending as great a breadth of the wheel, and length of buckets, as economical considerations will permit.

A bucket-wheel has been executed lately by Mr Robert Burns, at the cotton-mills of Houston, Burns, and Co. at Cartside in Renfrewshire, of a construction entirely new, but founded on a good principle, which is susceptible of great extension. It is represented in Fig. 8. The bucket consists of a start AB, an arm BC, and a wrest CD, concentric with the rim. But the bucket is also divided by a partition LM, concentric with the sole and rim, and so placed as to make the inner and outer portions of nearly equal capacity. It is evident, without any farther reasoning about it, that this partition will enable the bucket to retain its water much longer. When they are filled ⅓d, they retain the whole water at 18° from the bottom; and they retain ½ at 11°. They do not admit the water quite so freely as buckets of the common construction; but by means of the contrivance mentioned a little ago for the spout (also the invention of Mr Burns, and furnished with a rack-work, which raised or depressed it as the supply of water varied, so as at all times to employ the whole fall of the water), it is found that a slow-moving wheel allows one-half of the water to get into the inner buckets, especially if the partition do not altogether reach the radius drawn through the lip D of the outer bucket.

This is a very great improvement of the bucket-wheel; and when the wheel is made of a liberal breadth, so that the water may be very shallow in the buckets, it seems to carry the performance as far as it can go. Mr Burns made the first trial on a wheel of 24 feet diameter; and its performance is manifestly superior to that of the wheel which it replaced, and which was a very good one. It has also another valuable property: when the supply of water is very scanty, a proper adjustment of the apparatus in the spout will direct almost the whole of the water into the outer buckets; which, by placing it at a greater distance from the axis, makes a very sensible addition to its mechanical energy.

We said that this principle is susceptible of considerable extension; and it is evident that two partitions will increase the effect, and that it will increase with the number of partitions; so that when the practice now begun, of making water-wheels of iron, shall become general, and therefore very thin partitions are used, their number may be greatly increased without any inconvenience: and it is obvious, that this series of partitions must greatly contribute to the stiffness and general firmness of the whole wheel.

There frequently occurs a difficulty in the making of bucket-wheels, when the half-taught mill-wright attempts to retain the water a long time in the buckets. The water gets into them with a difficulty which he cannot account for, and spills all about, even when the buckets are not moving away from the spout. This arises from the air, which must find its way out to admit the water, but is obstructed by the entering water, and occasions a great sputtering at the entry. This may be entirely prevented by making the spout considerably narrower than the wheel. This will leave room at the two ends of the buckets for the escape of the air. This obstruction is vastly greater than one would imagine; for the water drags along with it a great quantity of air, as is evident in the *Water-blast* described by many authors.

There is another and very serious obstruction to the motion of an overshot or bucketed wheel. When it moves in back-water, it is not only resisted by the water when it moves more slowly than the wheel, which is very frequently the case, but it lifts a great deal in the rising buckets. In some particular states of back-water, the descending bucket fills itself completely with water; and, in other cases, it contains a very considerable quantity, and air of common density; while in some rarer cases it contains less water, with air in a condensed state. In the first case, the rising bucket must come up filled with water, which it cannot drop till its mouth get out of the water. In the second case, part of the water goes out before this; but the air rarefies, and therefore there is still some water dragged or lifted up by the wheel, by suction, as it is usually called. In the last case there is no such back load on the rising side of the wheel, but (which is as detrimental to its performance) the descending side is employed in condensing air; and although this air aids the ascent of the rising side, it does not aid it so much as it impedes the descending side, being (by the form of the bucket) nearer to the vertical line drawn through the axis.

All this may be completely prevented by a few holes made in the start of each bucket. Air being at least 800 times rarer than water, will escape through a hole almost 30 times faster with the same pressure. Very moderate holes will therefore suffice for this purpose: and the small quantity of water which these holes discharge during the descent of the buckets, produces a loss which is altogether insignificant. The water which runs out of one runs into another, so that there is only the loss of one bucket. We have seen a wheel of only 14 feet diameter working in nearly three feet of back-water. It laboured prodigiously, and great nicety; and authors, both speculative and practical, have entertained different, nay opposite, opinions on the subject. Mr Belidor, whom the engineers of Europe have long been accustomed to regard as sacred authority, maintains, that there is a certain velocity related to that obtainable by the whole fall, which will procure to an overshot wheel the greatest performance. Desaguiliers, Smeaton, Lambert, Des Parcieux, and others, maintain, that there is no such relation, and that the performance of an overshot wheel will be the greater, as it moves more slowly by an increase of its load of work. Belidor maintains, that the active power of water lying in a bucket-wheel of any diameter is equal to that of the impulse of the same water on the floats of an undershot wheel, when the water issues from a sluice in the bottom of the dam. The other writers whom we have named assert, that the energy of an undershot wheel is but one-half of that of an overshot, actuated by the same quantity of water falling from the same height.

To a manufacturing country like ours, which derives astonishing superiority, by which it more than compensates for the impediments of heavy taxes and luxurious living chiefly from its machinery, in which it leaves all Europe far behind, the decision of this question, in such a manner as shall leave no doubt or misconception in the mind even of an unlettered artist, must be considered as a material service; and we think that this is easily attainable.

When any machine moves uniformly, the accelerating force or pressure actually exerted on the impelled point of the machine is in equilibrio with all the resistances which are exerted at the working point with those arising from friction, and those that are excited in different parts of the machine by their mutual actions. This is an incontestable truth; and, though little attended to by the mechanicians, is the foundation of all practical knowledge of machines. Therefore, when an overshot wheel moves uniformly, with *any velocity whatever*, the water is acting with its whole weight: for gravity would accelerate its descent, if not completely balanced by some reaction; and in this balance gravity and the reacting part of the machine exert equal and opposite pressures, and thus produce the uniform motion of the machine. We are thus particular on this point, because we observe mechanicians of the first name employing a mode of reasoning on the question now before us which is specious, and appears to prove the conclusion which they draw; but is nevertheless contrary to true mechanical principles. They assert, that the slower a heavy body is descending (suppose in a scale suspended from an axis in peritrochea), the more does it press on the scale, and the more does it urge the machine round: and therefore the slower an overshot wheel turns, the greater is the force with which the water urges it round, and the more work will be done. It is very true that the machine is more forcibly impelled, and that more work is done: but this is not because a pound of water presses more strongly, but because there is more water pressing on the wheel; for the spout supplies at the same rate, and each bucket receives more water as it passes by it.

Let us therefore examine this question by the indubitable principles of mechanics.

Let the overshot wheel A*f*H (Fig. 5.) receive the water from a spout at the very top of the wheel; and, in order that the wheel may not be retarded by dragging into motion the water simply laid into the uppermost bucket at A, let it be received at B, with the velocity (directed in a tangent to the wheel) acquired by the head of water AP. This velocity, therefore, must be equal to that of the rim of the wheel. Let this be v, or let the wheel and the water move over v inches in a second. Let the buckets be of such dimensions, that all the water which each receives as it passes the spout is retained till it comes to the position R, where it is discharged at once. It is plain that, in place of the separate quantities of water lying in each bucket, we may substitute a continued ring of water, equal to their sum, and uniformly distributed in the space BER $\epsilon f \beta$. This constitutes a ring of uniform thickness. Let the area of its cross section β B or F f be called a. We have already demonstrated, that the mechanical energy with which this water on the circumference of the wheel urges it round, is the same with what would be exerted by the pillar $b\,r\,r\,b$ pressing on F f, or acting by the lever CF. The weight of this pillar may be expressed by $a \times b\,r$, or $a \times \mathrm{PS}$; and if we call the radius CF of the wheel R, the momentum or mechanical energy of this weight will be represented by $a \times \mathrm{PS} \times \mathrm{R}$.

Now, let us suppose that this wheel is employed to raise a weight W, which is suspended by a rope wound round the axis of the wheel. Let r be the radius of this axle. Then $\mathrm{W} \times r$ is the momentum of the work. Let the weight rise with the velocity u when the rim of the wheel turns with the velocity v, that is, let it rise u inches in a second.

Since a perfect equilibrium obtains between the power and the work when the motion is uniform, we must have $\mathrm{W} \times r = a \times \mathrm{PS} \times \mathrm{R}$. But it is evident that $\mathrm{R} : r = v : u$. Therefore $\mathrm{W} \times u = a \times v \times \mathrm{PS}$.

Now the performance of the machine is undoubtedly measured by the weight and the height to which it is raised in a second, or by $\mathrm{W} \times u$. Therefore the machine is in its best possible state when $a \times v \times \mathrm{PS}$ is a maximum. But it is plain that $a \times v$ is an invariable quantity; for it is the cubic inches of water which the spout supplies in a second. If the wheel moves fast, little water lies in each bucket, and a is small. When v is small, a is great, for the opposite reason; but $a \times v$ remains the same. Therefore we must make PS a maximum, that is, we must deliver the water as high up as possible. But this diminishes AP, and this diminishes the velocity of the wheel: and as this has no limit, the proposition is demonstrated; and an overshot wheel does the more work as it moves slowest.

Convincing as this discussion must be to any mechanician, we are anxious to impress the same maxim on the minds of practical men, unaccustomed to mathematical reasoning of any kind. We therefore beg indulgence for adding a popular view of the question, which requires no such investigation.

We may reason in this way: Suppose a wheel having 30 buckets, and that six cubic feet of water are delivered in a second on the top of the wheel, and discharged without any loss by the way at a certain height from the bottom of the wheel. Let this be the case, whatever is the rate of the wheel's motion; the buckets being of a sufficient capacity to hold all the water which falls into them. Let this wheel be employed to raise a weight of any kind, suppose water in a chain of 30 buckets, to the same height, and with the same velocity. Suppose, farther, that when the load on the rising side of the machine is one-half of that on the wheel, the wheel makes four times in a minute, or one turn in 15 seconds. During this time 90 cubic feet of water have flowed into the 30 buckets, and each has received three cubic feet. Then each of the rising buckets contains 1½ feet; and 45 cubic feet are delivered into the upper cistern during one turn of the wheel, and 180 cubic feet in one minute.

Now, suppose the machine so loaded, by making the rising buckets more capacious, that it makes only two turns in a minute, or one turn in 30 seconds. Then each descending bucket must contain six cubic feet of water. If each bucket of the rising side contained three cubic feet, the motion of the machine would be the same as before. This is a point which no mechanician will controvert. When two pounds are suspended to one end of a string which passes over a pulley, and one pound to the other end, the descent of the two pound will be the same with that of a four pounds weight, which is employed in the same manner to draw up two pounds. Our machine would therefore continue to make four turns in the minute, and would deliver 90 cubic feet during each turn, and 360 in a minute. But, by supposition, it is making but two turns in a minute: this must proceed from a greater load than three cubic feet of water in each rising bucket. The machine must therefore be raising *more* than 90 feet of water during one turn of the wheel, and *more* than 180 in the minute.

Thus it appears, that if the machine is turning twice as slow as before, there is *more than twice the former quantity* in the rising buckets, and more will be raised in a minute by the same expenditure of power. In like manner, if the machine go three times as slow, there must be *more than three times* the former quantity of water in the rising buckets, and more work will be done.

But we may go farther, and assert, that the *more* we retard the machine, by loading it with more work of a similar kind, the greater will be its performance. This does not immediately appear from the present discussion: but let us call the first quantity of water in the rising bucket A; the water raised by four turns in a minute will be $4 \times 30 \times A, = 120\,A$. The quantity in this bucket, when the machine goes twice as slow, has been shown to be greater than 2 A (call it $2\,A + x$); the water raised by two turns in a minute will be $2 \times 30 \times \overline{2\,A + x} = 120\,A + 60\,x$. Now, let the machine go four times as slow, making but one turn in a minute, the rising bucket must now contain more than twice $2\,A + x$, or more than $4\,A + 2\,x$; call it $4\,A + 2\,x + y$. The work done by one turn in a minute will now be $30 \times \overline{4\,A + 2\,x + y} = 120\,A + 60\,x + 30\,y$.

By such an induction of the work, done with any rates of motion we choose, it is evident that the performance of the machine increases with every diminution of its velocity that is produced by the mere addition of a similar load of work, or that it does the more work the slower it goes.

We have supposed the machine to be in its state of permanent uniform motion. If we consider it only in the beginning of its motion, the result is still more in favour of slow motion: for, at the first action of the moving power, the inertia of the machine itself consumes part of it, and it acquires its permanent speed by degrees; during which, the resistances arising from the work, friction, &c. increase, till they exactly balance the pressure of the water; and after this the machine accelerates no more. Now the greater the power and the resistance arising from the work are, in proportion to the inertia of the machine, the sooner will all arrive at its state of permanent velocity.

There is another circumstance which impairs the performance of an overshot wheel moving with a great velocity, *viz.* the effects of the centrifugal force on the water in the buckets. Our mill-wrights know well enough, that too great velocity will throw the water out of the buckets; but few, if any, know exactly the diminution of power produced by this cause. The following very simple construction will determine this: Let AOB (Fig. 10.) be an overshot wheel, of which AB is the upright diameter, and C is the centre. Make CF the length of a pendulum, which will make two vibrations during one turn of the wheel. Draw FE to the elbow of any of the buckets. The water in this bucket, instead of having its surface horizontal, as NO, will have it in the direction *n* O perpendicular to FE very nearly.

For the time of falling along half of FC is to that of two vibrations of this pendulum, or to the time of a revolution of the wheel as the radius of a circle is to its circumference: and it is well known, that the time of moving along half of AC, by the uniform action of the centrifugal force, is to that of a revolution as the radius of a circle to its circumference. Therefore the time of describing ½ of

AC by the centrifugal force, is equal to the time of describing ½ of FC by gravity. These spaces, being similarly described in equal times, are proportional to the accelerating forces. Therefore ½ FC : ½ AC, or FC : AC = gravity: centrifugal force. Complete the parallelogram FCEK. A particle at E is urged by its weight in the direction KE, with a force which may be expressed by FC or KE; and it is urged by the centrifugal force in the direction CE, with a force = AC or CE. By their combined action it is urged in the direction FE. Therefore, as the surface of standing water is always at right angles to the action of gravity, that is, to the plumb-line, so the surface of the water in the revolving bucket is perpendicular to the action of the combined force FE.

Let NEO be the position of the bucket, which just holds all the water which it received as it passed the spout when not affected by the centrifugal force; and let NDO be its position when it would be empty. Let the vertical lines through D and E cut the circle described round C with the radius CF in the points H and I. Draw HC, IC, cutting the circle AOB in L and M. Make the arch dδ equal to AL, and the arch dε equal to AM: Then Cδ and Cε will be the positions of the bucket on the revolving wheel, corresponding to CDO and CEO on the wheel at rest. Water will begin to run out at ε, and it will be all gone at δ.—The demonstration is evident.

The force which now urges the wheel is still the weight *really* in the buckets: for though the water is urged in the direction and with the force FE, one of its constituents, CE, has no tendency to impel the wheel; and KE is the only impelling force.

It is but of late years that mills have been constructed or attended to with that accuracy and scientific skill which are necessary for deducing confidential conclusions from any experiments that can be made with them; and it is therefore no matter of wonder that the opinions of mill-wrights have been so different on this subject. There is a natural wish to see a machine moving briskly; it has the appearance of activity: but a very slow motion always looks as if the machine were overloaded. For this reason mill-wrights have always yielded slowly, and with some reluctance, to the repeated advices of the mathematicians: but they have yielded; and we see them adopting maxims of construction more agreeable to sound theory; making their wheels of great breadth, and loading them with a great deal of work. Mr Euler says, that the performance of the best mill cannot exceed that of the worst above ⅛th: but we have seen a stream of water completely expended in driving a small flax-mill, which now drives a cotton-mill of 4000 spindles, with all its carding, roving, and drawing machinery, besides the lathes and other engines of the smiths and carpenters workshops, exerting a force not less than ten times what sufficed for the flax-mill.

The above discussion only demonstrates in general the advantages of slow motion; but does not point out in any degree the relation between the rate of motion and the work performed, nor even the principles on which it depends. Yet this is a subject fit for a mathematical investigation; and we would prosecute it in this place, if it were necessary for the improvement of practical mechanics. But we have seen that there is not, in the nature of things, a maximum of performance attached to any particular rate of motion which should therefore be preferred. For this reason we omit this discussion of mere speculative curiosity. It is very intricate: for we must not now express the pressure on the wheel by a *constant* pillar of water incumbent on the extremity of the horizontal arm, as we did before when we supposed the buckets completely filled; nor by a smaller *constant* pillar, corresponding to a smaller but equal quantity lying in every bucket. Each different velocity puts a different quantity of water into the bucket as it passes the spout; and this occasions a difference in the place where the discharge is begun and completed. This circumstance is some obstacle to the advantages of very slow motions, because it brings on the discharge sooner. All this may indeed be expressed by a simple equation of easy management; but the whole process of the mechanical discussion is both intricate and tedious, and the results are so much diversified by the forms of the buckets, that they do not afford any rule of sufficient generality to reward our trouble. The curious reader may see a very full investigation of this subject in two dissertations by Elvius in the Swedish Transactions, and in the *Hydrodynamique* of Professor Karstner of Gottingen; who has abridged these Dissertations of Elvius, and considerably improved the whole investigation, and has added some comparisons of his deductions with the actual performance of some great works. These comparisons, however, are not very satisfactory. There is also a valuable paper on this subject by Mr Lambert, in the Memoirs of the Academy of Berlin for the year 1775. From these dissertations, and from the *Hydrodynamique* of the Abbé Bossut, the reader will get all that theory can teach of the relation between the pressures of the power and work on the machine and the rates of its motion. The practical reader may rest with confidence on the simple demonstration we have given, that the performance is improved by diminishing the velocity.

All we have to do, therefore, is to load the machine, and thus to diminish its speed, unless other physical circumstances throw obstacles in the way: but there are such obstacles. In all machines there are little inequalities of action that are unavoidable. In the action of a wheel and pinion, though made with the utmost judgment and care, there are such inequalities. These increase by the changes of form occasioned by the wearing of the machine—much greater irregularities arise from the subsultory motions of cranks, stampers, and other parts which move unequally or reciprocally. A machine may be so loaded as just to be in equilibrio with its work, in the favourable position of its parts. When this changes into one less favourable, the machine may stop; if not, it at least staggers, hobbles, or works unequally. The rubbing parts bear long on each other, with enormous pressures, and cut deep, and increase friction. Such slow motions must therefore be avoided. A little more velocity enables the machine to get over those increased resistances by its inertia, or the great quantity of motion inherent in it. Great machines possess this advantage in a superior degree, and will therefore work steadily with a smaller velocity. These circumstances are hardly susceptible of mathematical discussion, and our best reliance is on well-directed experience.

For this purpose, the reader will do well to peruse with care the excellent paper by Mr Smeaton in the Philosophical Transactions for 1759. This dissertation contains a numerous list of experiments, most judiciously contrived by him, and executed with the accuracy and attention, to the most important circumstances, which is to be observed in all that gentleman's performances.

It is true, these experiments were made with small models; and we must not, without great caution, transfer the results of such experiments to large works. But we may safely transfer the *laws* of variation which result from a variation of circumstances, although we must not adopt the absolute quantities of the variations themselves. Mr Smeaton was fully aware of the limitations to which conclusions drawn from experiments on models are subject, and has made the applications with his usual sagacity.

His general inference is, that, in smaller works, the rim of the overshot wheel should not have a greater velocity than three feet in a second; but that larger mills may be allowed a greater velocity than this. When every thing is executed in the best manner, he says that the work performed will amount to fully two-thirds of the power expended; that is, that three cubic feet of water descending from any height, will raise two to the same height.

It is not very easy to compare these deductions with observations on large works; because there are few cases where we have good measures of the resistances opposed by the work performed by the machine. Mills employed for pumping water afford the best opportunities. But the inertia of their working gear diminishes their useful performance very sensibly; because their great beams, pump-rods, &c. have a reciprocating motion, which must be destroyed, and produced anew in every stroke. We have examined some machines of this kind which are esteemed good ones; and we find few of them whose performance exceeds one-half of the power expended.

By comparing other mills with these, we get the best information of their resistances. The comparison with mills worked by Watt and Boulton's steam-engines is perhaps a better measure of the resistances opposed by different kinds of work, because their power is very distinctly known. We have been informed by one of the most eminent engineers, that a ton and half of water *per* minute falling one foot will grind and dress one bushel of wheat *per* hour. This is equivalent to 9 tons falling 10 feet.

If an overshot wheel opposed no resistance, and only *one* bucket were filled, the wheel would acquire the velocity due to a fall through the whole height. But when it is in this state of accelerated motion, if another bucket of water is delivered into it, its motion must be checked at the first, by the necessity of dragging forward this water. If the buckets fill in succession as they pass the spout, the velocity acquired by an unresisting wheel is but half of that which one bucket would give. In all cases, therefore, the velocity is diminished by the inertia of the entering water when it is simply laid into the upper buckets. The performance will therefore be improved by delivering the water on the wheel with that velocity with which the wheel is really moving. And as we cannot give the direction of a tangent to the wheel, the velocity with which it is delivered on the wheel must be so much greater than the intended velocity of the rim, that it shall be precisely equal to it when it is estimated in the direction of the tangent. Three or four inches of fall are sufficient for this purpose; and it should never be neglected, for it has a very sensible influence on the performance. But it is highly improper to give it more than this, with the view of impelling the wheel by its stroke. For even although it were proper to employ part of the fall in this way (which we shall presently see to be very improper), we cannot procure this impulse; because the water falls among other water, or it strikes the boards of the wheel with such obliquity that it cannot produce any sensible effect.

It is a much-debated question among mill-wrights, Whether the diameter of the wheel should be such as that the water will be delivered at the top of the wheel; or larger, so that the water is received at some distance from the top, where it will act more perpendicularly to the arm? We apprehend that the observations formerly made will decide in favour of the first practice. The space below, where the water is discharged from the wheel, being proportional to the diameter of the wheel, there is an undoubted loss of fall attending a large wheel; and this is not compensated by delivering the water at a greater distance from the perpendicular. We should therefore recommend the use of the whole descending side, and make the diameter of the wheel no greater than the fall, till it is so much reduced that the centrifugal force begins to produce a sensible effect. Since the rim can hardly have a smaller velocity than three feet *per* second, it is evident that a small wheel must revolve more rapidly. This made it proper to insert the determination that we have given, of the loss of power produced by the centrifugal force. But even with this in view, we should employ much smaller wheels than are generally done on small falls. Indeed the loss of water at the bottom may be diminished, by nicely fitting the arch which surrounds the wheel, so as not to allow the water to escape by the sides or bottom. While this improvement remains in good order, and the wheel entire, it produces a very sensible effect; but the passage widens continually by the wearing of the wheel. A bit of stick or stone falling in about the wheel tears off part of the shrouding or bucket, and frosty weather frequently binds all fast. It therefore seldom answers expectations. We have nothing to add on this case to what we have already extracted from Mr Smeaton's Dissertation on the Subject of Breast or half Overshot Wheels.

There is another form of wheel by which water is made to act on a machine by its weight, which merits consideration. This is known in this country by the name of *Barker's mill*, and has been described by Desaguiliers, vol. II. p. 468. It consists of an upright pipe or trunk AB (Fig. 11.), communicating with two horizontal branches BC, B *c*, which have a hole C *c* near their ends, opening in opposite directions, at right angles to their lengths. Suppose water to be poured in at the top from the spout F, it will run out by the holes C and *c* with the velocity corresponding to the depth of these holes under the surface. The consequence of this must be, that the arms will be pressed backwards; for there is no solid surface at the hole C, on which the lateral pressure of the water can be exerted, while it acts with its full force on the opposite side of the arm. This unbalanced pressure is equal to the weight of a column having the orifice for its base, and twice the depth under the surface of the water in the trunk for its height. This measure of the height may seem odd, because if the orifice were shut, the pressure on it is the weight of a column reaching from the surface. But when it is open, the water issues with nearly the velocity acquired by falling from the surface, and the quantity of motion produced is that of a column of twice this length, moving with this velocity. This is actually produced by the pressure of the fluid, and must therefore be accompanied by an equal reaction.

Now suppose this apparatus set on the pivot E, and to have a spindle AD above the trunk, furnished with a cylindrical bobbin D, having a rope wound round it, and passing over a pulley G. A weight W may be suspended there, which may balance this backward pressure. If the weight be too small for this purpose, the retrograde motion of the arms will wind up the cord, and raise the weight; and thus we obtain an acting machine, employing the pressure of the water, and applicable to any purpose. A runner millstone may be put on the top of the spindle; and we should then produce a flour-mill of the utmost simplicity, having neither wheel nor pinion, and subject to hardly any wear. It is somewhat surprising, that although this was invented at the beginning of this century, and appears to have such advantage in point of simplicity, it has not come into use. So little has Dr Desaguilier's account been attended to (although it is mentioned by him as an excellent machine, and as highly instructive to the hydraulist), that the same invention was again brought forward by a German professor (Segner) as his own, and has been honoured by a series of elaborate disquisitions concerning its theory and performance by Euler and by John Bernoulli. Euler's dissertations are to be found in the Memoirs of the Academy of Berlin, 1751, &c. and in the *Nov. Comment. Petropol.* tom. VI. Bernoulli's are at the end of his Hydraulics. Both these authors agree in saying, that this machine excels all other methods of employing the force of water. Simple as it appears, its true theory, and the best form of construction, are most abstruse and delicate subjects; and it is not easy to give such an account of its principles as will be understood by an ordinary reader.

We see, in general, that the machine must press backwards; and little investigation suffices for understanding the intensity of this pressure when the machine is at rest. But when it is allowed to run backwards, withdrawing itself from the pressure, the intensity of it is diminished: and if no other circumstances intervened, it might not be difficult to say what particular pressure corresponded to any rate of motion. Accordingly, Desaguiliers, presuming on the simplicity of the machine, affirms the pressure to be the weight of a column, which would produce a velocity of efflux equal to the difference of the velocity of the fluid and of the machine; and hence he deduces, that its performance will be the greatest possible, when its retrograde velocity is one-third of the velocity acquired by falling from the surface, in which case, it will raise $\frac{8}{27}$ths of the water expended to the same height, which is double of the performance of a mill acted on by the impulse of water.

But this is a very imperfect account of the operation. When the machine (constructed exactly as we have described) moves round, the water which issues descends in the vertical trunk, and then, moving along the horizontal arms, partakes of this circular motion. This excites a centrifugal force, which is exerted against the ends of the arms by the intervention of the fluid. The whole fluid is subjected to this pressure (increasing for every section across the arm in the proportion of its distance from the axis), and every particle is pressed with the accumulated centrifugal forces of all the sections that are nearer to the axis. Every section therefore sustains an actual pressure proportional to the square of its distance from the axis. This increases the velocity of efflux, and this increases the velocity of revolution; and this mutual co-operation would seem to terminate in an infinite velocity of both motions.

But, on the other hand, this circular motion must be given anew to every particle of water as it enters the horizontal arm. This can be done only by the motion already in the arm, and at its expense. Thus there must be a velocity which cannot be overpassed even by an unloaded machine. But it is also plain, that by making the horizontal arm very capacious, the motion of the water from the axis to the jet may be made very slow, and much of this diminution of circular motion prevented. Accordingly, Euler has recommended a form by which this is done in the most eminent degree. His machine consists of a hollow conoidal ring, of which Fig. 12. is a section. The part AH *ha* is a sort of funnel basin, which receives the water from the spout F; not in the direction pointing towards the axis, but in the direction, and with the precise velocity, of its motion. This prevents any retardation by dragging forward the water. The water then passes down between the outer conoid AC *ca* and the inner conoid HG *gh* along spiral channels formed by partitions soldered to both conoids. The curves of these channels are determined by a theory which aims at the annihilation of all unnecessary and improper motions of the water, but which is too abstruse to find a place here. The water thus conducted arrives at the bottom CG, *cg*. On the outer circumference of this bottom are arranged a number of spouts (one for each channel), which are all directed one way in tangents to the circumference.

Adopting the common theory of the reaction of fluids, this should be a very powerful machine, and should raise $\frac{8}{27}$ths of the water expended. But if we admit the reaction to be equal to the force of the issuing fluid (and we do not see how this can be refused), the machine must be nearly twice as powerful. We therefore repeat our wonder, that it has not been brought into use. But it appears that no trial has been made even of a model; so that we have no experiments to encourage an engineer to repeat the trial.

Even the late author, Professor Segner, has not related any thing of this kind in his *Exercitationes Hydraulicæ*, where he particularly describes the machine. This remissness probably has proceeded from fixing the attention on Euler's improved construction. It is plain that this must be a most cumbrous mass, even in a small size, requiring a prodigious vessel, and carrying an unwieldy load. If we examine the theory which recommends this construction, we find that the advantages, though real and sensible, bear but a small proportion to the whole performance of the simple machine as invented by Dr Barker. It is therefore to be regretted, that engineers have not attempted to realize the first project. We beg leave to recommend it, with an additional argument taken from an addition made to it by Mr Mathon de la Cour, in Rozier's *Journal de Physique*, January and August 1775. This gentleman brings down a large pipe FEH (Fig. 13.) from a reservoir, bends it upward at H, and introduces it into two horizontal arms DA, DB, which have an upright spindle DK, carrying a millstone in the style of Dr Barker's mill. The ingenious mechanician will have no difficulty of contriving a method of joining these pipes, so as to permit a free circular motion without losing much water. The operation of the machine in this form is evident. The water, pressed by the column FG, flows out at the holes A and B, and the unbalanced pressure on the opposite sides of the arms forces them round. The compendiousness and other advantages of this construction are most striking, allowing us to make use of the greatest fall without any increase of the size of the machine. It undoubtedly enables us to employ a stream of water too scanty to be employed in any other form. The author gives the dimensions of an engine which he had seen at Bourg Argental. AB is 92 inches, and its diameter 3 inches; the diameter of each orifice is 1⅛; FG is 21 feet; the pipe D was fitted into C by grinding; and the internal diameter of D is 2 inches.

When the machine was performing no work, or was unloaded, and emitted water by one hole only, it made 115 turns in a minute. This gives a velocity of 46 feet *per* second for the hole. This is a curious fact: for the water would issue from this hole at rest with the velocity of 37¼. This great velocity (which was much less than the velocity with which the water actually quitted the pipe) was undoubtedly produced by the prodigious centrifugal force, which was nearly 17 times the weight of the water in the orifice.

The empty machine weighed 80 pounds, and its weight was half supported by the upper pressure of the water, so that the friction of the pivots was much diminished. It is a pity that the author has given no account of any work done by the machine. Indeed it was only working ventilators for a large hall. His theory by no means embraces all its principles, nor is it well-founded.

We think that the free motion round the neck of the feeding pipe, without any loss of water or any considerable friction, may be obtained in the following manner: AB (Fig. 14.) represents a portion of the revolving horizontal pipe, and CE *e c* part of the feeding pipe. The neck of the first is turned truly cylindrical, so as to turn easily, but without shake, in the collar C *c* of the feeding pipe, and each has a shoulder which may support the other. That the friction of this joint may not be great, and the pipes destroy each other by wearing, the horizontal pipe has an iron spindle EF, fixed exactly in the axis of the joint, and resting with its pivot F in a step of hard steel, fixed to the iron bar GH, which goes across the feeding pipe, and is firmly supported in it. This pipe is made bell-shaped, widening below. A collar or hose of thin leather is fitted to the inside of this pipe, and is represented (in section) by LKM *m k l*. This is kept in its place by means of a metal or wooden ring N *n*, thin at the upper edge, and taper-shaped. This is drawn in above the leather, and stretches it, and causes it to apply to the side of the pipe all around. There can be no leakage at this joint, because the water will press the leather to the smooth metal pipe; nor can there be any sensible friction, because the water gets at the edge of the leather, and the whole unbalanced pressure is at the small crevice, between the two metal shoulders. These shoulders need not touch, so that the friction must be insensible. We imagine that this method of tightening a turning joint may be used with great advantage in many cases.

We have only further to observe on this engine, that any imperfection by which the passage of the water is diminished or obstructed produces a saving of water which is in exact proportion to the diminution of effect. The only inaccuracy that is not thus compensated is when the jets are not at right angles to the arms.

We repeat our wishes, that engineers would endeavour to bring this machine into use, seeing many situations where it may be employed to great advantage. Suppose, for instance, a small supply of water from a great height applied in this manner to a centrifugal pump, or to a hair belt passing over a pulley, and dipping in the water of a deep well. This would be a hydraulic machine exceeding all others in simplicity and durability, though inferior in effect to some other constructions.

2. *Of Undershot Wheels.*

ALL wheels go by this name where the motion of the water is quicker than that of the partitions or boards of the wheel, and it therefore impels them. These are called the *float-boards*, or *floats*, of an undershot wheel. The water, running in a mill-row, with a velocity derived from a head of water, or from a declivity of channel, strikes on these floats, and occasions, by its deflections sidewise and upwards, a pressure on the floats sufficient for impelling the wheel.

There are few points of practical mechanics that have been more considered than the action of water on the floats of a wheel, hardly a book of mechanics being silent on the subject. But the generality of them, at least such as are intelligible to persons who are not very much conversant in dynamical and mathematical discussion, have hardly done any thing more than copied the earliest deductions from the simple theory of the resistance of fluids. The consequence has been, that our practical knowledge is very imperfect; and it is still chiefly from experience that we must learn the performance of undershot wheels. Unfortunately this stops their improvement; because those who have the only opportunities of making the experiments are not sufficiently acquainted with the principles of hydraulics, and are apt to ascribe differences in their performance to trifling nostrums in their construction, or in the manner of applying the impulse of the water.

We have said so much on the imperfection of our theories of the impulse of fluids in the article *Resistance of Fluids*, that we need not repeat here the defects of the common explanations of the motions of undershot wheels. The part of this theory of the impulse of fluids which agrees best with observation is, *that the impulse is in the duplicate proportion of the velocity with which the water strikes the float*. That is, if v be the velocity of the stream, and u the velocity of the float, we shall have F, the impulse on the float when held fast to its impulse f on the float moving with the velocity u, as v^2 to $\overline{v-u}^2$, and $f = F \times \frac{\overline{v-u}^2}{v^2}$.

This is the pressure acting on the float, and urging the wheel round its axis. The wheel must yield to this motion, if the resistance of the work does not exert a superior pressure on the float in the opposite direction. By yielding, the float withdraws from the impulse, and this is therefore diminished. The wheel accelerates, the resistances increase, and the impulses diminish, till they become an exact balance for the resistances. The motion now remains uniform, and the momentum of impulse is equal to that of resistance. The performance of the mill therefore is determined by this; and, whatever be the construction of the mill, its performance is best when the momentum of impulse is greatest. This is had by multiplying the pressure on the float by its velocity. Therefore the momentum will be expressed by $F \times \frac{\overline{v-u}^2}{v^2} \times u$. But since F and v^2 are constant quantities, the momentum will be proportional to $u \times \overline{v-u}^2$. Let x represent the relative velocity. Then $v-x$ will be $= u$, and the momentum will be proportional to $\overline{v-x} \times x^2$, and will be a maximum when $\overline{v-x} \times x^2$ is a maximum, or when $v x^2 - x^3$ is a maximum. This will be discovered by making its fluxion $= o$. That is,

$$2 v x \dot{x} - 3 \dot{x}^2 x = o.$$

and $2 v x - 3 x^2 = o$

or $2 v - 3 x = o$

and $2 v = 3 x$, and $x = \frac{2}{3} v$; and therefore $v - x$, or $u, = \frac{1}{3} v$. That is, the velocity of the float must be one-third of the velocity of the stream. It only remains to say what is the absolute pressure on the float thus circumstanced. Let the velocity v be supposed to arise from the pressure of a head of water h. The common theory teaches that the impulse on a given surface S at rest is equal to the weight of a column h S; put this in place of F, and $\frac{4}{9} v^2$ in place of $\overline{v-u}^2$ and $\frac{1}{3} v$ for u. This gives us $S h \times \frac{4}{27} v$ for the momentum. Now the power expended is $S h v$, or the column $S h$ moving with the velocity v. Therefore the greatest performance of an undershot wheel is equivalent to raising $\frac{4}{27}$ of the water that drives it to the same height.

But this is too small an estimation; for the pressure exerted on a plane surface, situated as the float in a mill-wheel, is considerably greater than the weight of the column S *A*. This is nearly the pressure on a surface wholly immersed in the fluid. But when a small vein strikes a larger plane, so as to be deflected on all sides in a thin sheet, the impulse is almost double of this. This is in some measure the case in a mill-wheel. When the stream strikes it, it is heaped up along its face, and falls back again—and during this motion it is acting with a hydrostatic pressure on it. When the wheel dips into an open river, this accumulation is less remarkable, because much escapes laterally; but in a mill-course it may be considerable.

We have considered only the action on one float, but several generally act at once. The impulse on most of them must be oblique, and is therefore less than when the same stream impinges perpendicularly; and this diminution of impulse is, by the common theory, in the proportion of the sine of the obliquity. For this reason it is maintained, the impulse of the whole stream on the lowest float-board, which is perpendicular to the stream, is equal to the sum of the impulses made on all the floats which then dip into the water; or that the impulse on any oblique float is precisely equal to the impulse which that part of the stream would have made on the lowest float-board had it not been interrupted. Therefore it has been recommended to make such a number of float-boards, that when one of them is at the bottom of the wheel, and perpendicular to the stream, the next in succession should be just entering into the water. But since the impulse on a float by no means annihilates all the motion of the water, and it bends round it and hits the one behind with its remaining force, there must be some advantage gained by employing a greater number of floats than this rule will permit. This is abundantly confirmed by the experiments of Smeaton and Bossut. M. Bossut formed three or four suppositions of the number of floats, and calculated the impulse on each; according to the observations made in a course of experiments made by the Academy of Sciences, and inserted by us in the article Resistance of Fluids; and when he summed them up and compared the results with his experiments, he found the agreement very satisfactory. He deduces a general rule, that if the velocity of the wheel is ⅓d of that of the stream, and if 72 degrees of the circumference are immersed in the stream, the wheel should have 36 floats. Each will dip ⅛th of the radius. The velocity being still supposed the same, there should be more or fewer floats according as the arch is less or greater than 72 degrees.

Such is the theory, and such are the circumstances which it leaves undetermined. The accumulation of the water on a float-board, and the force with which it may still strike another, are too intricate to be assigned with any tolerable precision: for such reasons we must acknowledge that the theory of undershot wheels is still very imperfect, and that recourse must be had to experience for their improvement. We therefore strongly recommend the perusal of Mr Smeaton's experiments on undershot wheels, contained in the same dissertation with those we have quoted on overshot wheels. We have only to observe, that to an ordinary reader the experiments will appear too much in favour of undershot wheels. His aim is partly to establish a theory, which will state the relation between their performance and the velocity of the stream, and partly to state the relation between the power expended and the work done. The velocity in his experiments is always considerably below that which a body would acquire by falling from the surface of the head of water; or it is the velocity acquired by a shorter fall. Therefore, if we estimate the power expended by the quantity of water multiplied by this diminished fall, we shall make it too small; and the difference in some cases is very great: yet, even with these concessions, it appears that the utmost performance of an undershot wheel does not surpass the raising $\frac{1}{3}$d of the expended water to the place from which it came. It is therefore far inferior to an overshot wheel expending the same power; and Mr Belidor has led engineers into very mistaken maxims of construction, by saying that overshot wheels should be given up, even in the case of great falls, and that we should always bring on the water from a sluice in the very bottom of the dam, and bring it to the wheel with as great velocity as possible. Mr Smeaton also says, that the maximum takes place when the velocity of the wheel is $\frac{2}{5}$ths of that of the stream, instead of $\frac{1}{3}$ths according to the theory; and this agrees with the experiments of Bossut. But he measured the velocity by means of the quantity of water which run past. This must give a velocity somewhat too small, as will appear by attending to Buat's observations on the superficial, the mean, and the bottom velocities.

The rest of his observations, are most judicious, and well adapted to the instruction of practitioners. We have only to add to them the observations of Deparcieux and Bossut, who have evinced, by very good experiments, that there is a very sensible advantage gained by inclining the float-boards to the radius of the wheel about 20 degrees, so that the lowest float-board shall not be perpendicular, but have its point turned up the stream about 20 degrees. This inclination causes the water to heap up along the float-board, and act by its weight. The floats should therefore be made much broader than the vein of water interrupted by them is deep.

Some engineers, observing the great superiority of overshot wheels above undershot wheels driven by the same expense of power, have proposed to bring the water home to the bottom of the wheel on an even bottom, and to make the float-board no deeper than the aperture of the sluice, which would permit the water to run out. The wheel is to be fitted with a close sole and sides, exactly fitted to the end of this trough, so that if the wheel is at rest, the water may be dammed up by the sole and float-board. It will therefore press forward the float-board with the whole force of the head of water. But this cannot answer: for if we suppose no float-boards, the water will flow out at the bottom, propelled in the manner those persons suppose; and it will be supplied from behind, the water coming *slowly* from all parts of the trough to the hole below the wheel. But now add the floats, and suppose the wheel *in* motion with the velocity that is expected. The other floats must drag into motion all the water which lies between them, giving to the greatest part of it a motion vastly greater than it would have taken in consequence of the pressure of the water behind it; and the water out of the reach of the floats will remain still, which it would not have done independent of the float-boards above it, because it would have contributed to the expense of the hole. The motion, therefore, which the wheel will acquire by this construction must be so different from what is expected, that we can hardly say what it will be.

We are therefore persuaded, that the best way of delivering the water on an undershot wheel in a close mill-course is, to let it slide down a very smooth channel, without touching the wheel till near the bottom, where the wheel should be exactly fitted to the course; or, to make the floats exceedingly broader than the depth of the vein of water which glides down the course, and allow it to be partly intercepted by the first floats, and heap up along them, acting by its weight, after its impulse has been expended. If the bottom of the course be an arch of a circle described with a radius much greater than that of the wheel, the water which slides down will be thus gradually intercepted by the floats.

Attempts have been made to construct water-wheels which receive the impulse obliquely, like the sails of a common wind-mill. This would, in many situations, be a very great acquisition. A very slow but deep river could in this manner be made to drive our mills; and although much power is lost by the obliquity of the impulse, the remainder may be very great. It is to be regretted, that these attempts have not been more zealously prosecuted; for we have no doubt of their success in a very serviceable degree. Engineers have been deterred, because when such wheels are plunged in an open stream, their lateral motion is too much impeded by the motion of the stream. We have seen one, however, which was very powerful: it was a long cylindrical frame, having a plate standing out from it about a foot broad, and surrounding it with a very oblique spiral like a cork-screw. This was plunged about one-fourth of its diameter (which was about 12 feet), having its axis in the direction of the stream. By the work which it was performing, it seemed more powerful than a common wheel which occupied the same *breadth* of the river. Its length was not less than 20 feet: it might have been twice as much, which would have doubled its power, without occupying more of the water-way. Perhaps such a spiral, continued to the very axis, and moving in a hollow canal wholly filled by the stream, might be a very advantageous way of employing a deep and slow stream.

But mills with oblique floats are most useful for employing small streams, which can be delivered from a spout with a great velocity. Mr Bossut has considered these with due attention, and ascertained the best modes of construction. There are two which have nearly equal performances: 1. The vanes being placed like those of a windmill, round the rim of a horizontal or vertical wheel, and being made much broader than the vein of water which is to strike them, let the spout be so directed that the vein may strike them perpendicularly. By this measure it will be spread about on the vane in a thin sheet, and exert a pressure nearly equal to twice the weight of a column whose base is the orifice of the spout, and whose height is the fall producing the velocity.

Mills of this kind are much in use in the south of Europe. The wheel is horizontal, and the vertical axis carries the millstone; so that the mill is of the utmost simplicity: and this is its chief recommendation; for its power is greatly inferior to that of a wheel constructed in the usual manner.

2. The vanes may be arranged round the rim of the wheel, not like the sails of a wind-mill, in planes inclined to the radii, but parallel to the axis, or to the planes passing through the axis. They may either stand on a sole, like the oblique floats recommended by De Parceux, as above-mentioned; or they may stand on the side of the rim, not pointing to the axis, but aside from it.

This disposition will admit the spout to be more conveniently disposed either for a horizontal or a vertical wheel.

We shall conclude this article by describing a contrivance of Mr Burns, the inventor of the double-bucketed wheel, for fixing the arms of a water-wheel. It is well known to mill-wrights, that the method of fixing them by making them to pass through the axle, weakens it exceedingly, and by lodging water in the joint, soon causes it to rot and fail. They have, therefore, of late years put cast-iron flanches on the axis, to which each arm is bolted: or the flanches are so fashioned as to form boxes, serving as mortices to receive the ends of the arms. These answer the purpose completely, but are very expensive; and it is found that arms of fir, bolted into flanches of iron, are apt to work loose. Mr Burns has made wooden flanches of a very curious construction, which are equally firm, and cost much less than the iron ones.

This flanch consists of eight pieces, four of which compose the ring represented in Fig. 15. meeting in the joints *a b*, *a b*, *a b*, *a b*, directed to the centre O. The other four are covered by these, and their joints are represented by the dotted lines $\alpha\beta$, $\alpha\beta$, $\alpha\beta$, $\alpha\beta$. These two rings break joint in such a manner that an arm MN is contained between the two nearest joints $a'b'$ of the one, and $\alpha'\beta'$ of the other. The tenon formed on the end of the arm A, &c. is of a particular shape: one side, GF is directed to the centre O; the other side, BCDE, has a small shoulder BC; then a long side CD directed to the centre O; and then a third part DE parallel to GF, or rather diverging a little from it, so as to make up at E the thickness of the shoulder BC; that is, a line from B to E would be parallel to CD. This side of the tenon fits exactly to the corresponding side of the mortise; but the mortise is wider on the other side, leaving a space GFK*h* a little narrower at FK than at G*h*. These tenons and mortises are made extremely true to the square; the pieces are put round the axle, with a few blocks or wedges of soft wood put between them and the axle, leaving the space empty opposite to the place of each arm, and firmly bolted together by bolts between the arm mortises. The arms are then put in, and each is pressed home to the side CDE, and a wedge HF of hard wood is then put into the empty part of the mortise and driven home. When it comes through the flanch and touches the axle, the part which has come through is cut off with a thin chisel, and the wedge is driven better home. The spaces under the ends of the arms are now filled with wedges, which are driven home from opposite sides, till the circle of the arms stands quite perpendicular on the axle, and all is fast. It needs no hoops to keep it together, for the wedging it up round the axle makes the two half rings draw close on the arms, and it cannot start at its own joints till it crushes the arms. Hoops, however, can do no harm, when all is once wedged up, but it would be improper to put them on before this be done.

PUMP.

It would be an entertaining and not an uninstructive piece of information to learn the progressive steps by which the ingenuity of man has invented the various methods of raising water. A pump must be considered as the last step of this progress. Common as it is, and overlooked even by the curious, it is a very abstruse and refined invention. Nothing like it has been found in any of the rude nations whom the restless spirit of the Europeans has discovered, either in the new continent of America or the islands of the Pacific Ocean. Nay, it was unknown in the cultivated empire of China at the time of our arrival there by sea; and it is still a rarity everywhere in Asia, in places unfrequented by the Europeans. It does not appear to have been known to the Greeks and Romans in early times; and perhaps it came from Alexandria, where physical and mathematical science was much cultivated by the Greek school under the protection of the Ptolemies. The performances of Ctesibius and Hero are spoken of by Pliny and Vitruvius as curious novelties.* It is perhaps not difficult to trace the steps by which those mechanicians were led to the invention. The Egyptian wheel was a common machine all over Asia, and is still in use in the remotest corners, and was brought by the Saracens into Spain, where it is still very common under its ancient name NORIA. The Danish missionaries found in a remote village in the kingdom of Siam the immediate offspring of the noria. It was a wheel turned by an ass, and carrying round, not a string of earthen pots, but a string of wisps of hay which it drew through a wooden trunk. This rude chain-pump was in frequent use for watering the rice-fields. It is highly probable that it is of great antiquity, although we do not recollect its being mentioned by any of the Greek or Roman writers. The Arabs and Indians were nothing less than innovators; and we may suppose with great safety, that what arts we now find among them they possessed in very remote periods. Now the step from this to the pump is but short, though it is nice and refined; and the forcing pump of Ctesibius is the easiest and most natural.

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* In the early Greek writings, it does not appear that the words 'αντλος, 'αντλᾶν, 'αντλια, &c. were used to express any thing like what we call a pump. In all these passages the words either express generally the drawing of water, or, more particularly, the drawing it with

Let AB (Plate XII. Fig. 1.) be the surface of the water in the well, and D the height where it is to be delivered. Let DC be a long wooden trunk, reaching as deep under water as possible. Let the rope EF be fitted with its knot of hay F. When it is drawn up through the

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a bucket or something similar. 'Αντλος, which is the primitive, is a drain, sink, or receptacle for collecting scattered water, either for use, or to get rid of it; hence it came to signify the sink or well of a ship; and 'αντλᾶν was synonimous with our verb "to bale the boat." (*Odyss.* O. 476. M. 411. *Euryp. Hecuba*, 1025.) 'Αντλιον is the *vessel* or *bucket* with which water is drawn. 'Αντλια is the service (generally a punishment) of drawing water. Αντλᾶν, "to draw water with a bucket:" hence the force of Aristotle's expression (*Oecon.* 1.), τω γαρ ἡμᾶ 'αντλιν τουτ' ἐςι. See even the late authority of the New Testament, John ii. 8.; iv. 7. 11. Here 'αντλημα is evidently something which the woman brought along with her; probably a bucket and rope.

trunk, it will bring up along with it all the water lying between C and A, which will begin to run out by the spout D as soon as the knot gets to G, as far below D as C is below A. All this is very obvious; and it required but little reflection to be assured, that if F was let down again, or pushed down, by a rod instead of a rope, it would again perform the same office. Here is a very simple pump. And if it was ever put in practice, it behoved to show the supporting power of the atmosphere, because the water would not only be lifted by the knot, but would even follow it. The imperfection of this pump behoved to appear at first sight, and to suggest its remedy. By pushing down the knot F, which we shall henceforward call the *piston*, all the force expended in lifting up the water between A and G is thrown away, because it is again let down. A valve G, at the bottom, would prevent this. But then there must be a passage made for the water by a lateral tube KBD (Fig. 2.) And if this be also furnished with a valve H, to prevent its losing the water, we have the pump of Ctesibius, as sketched in Fig. 2. The valve is the great refinement: but perhaps even this had made its appearance before in the noria. For, in the more perfect kinds of these machines, the pots have a stop or valve in their bottom, which hangs open while the pot descends with its mouth downwards, and then allows it to fill readily in the cistern; whereas, without the valve, it would occasion a double load to the wheel. If we suppose that the valve had made its appearance so early, it is not improbable that the common pump sketched in Fig. 3. was as old as that of Ctesibius. In this dissertation we shall first give a short description of the chief varieties of these engines, considering them in their simplest form, and we shall explain in very general terms their mode of operation. We shall then give a concise and popular theory of their operation, furnishing principles to direct us in their construction; and we shall conclude with the description of a few peculiarities which may contribute to their improvement or perfection.

There are but two sorts of pumps which essentially differ; and all the varieties that we see are only modifications of these. One of these original pumps has a solid piston; the other has a piston with a perforation and a valve. We usually call the first a FORCING PUMP, and the second a LIFTING or SUCKING PUMP.

Fig. 2. is a sketch of the forcing pump in its most simple form and situation. It consists of a hollow cylinder AC*ca*, called the WORKING BARREL, open at both ends, and having a valve G at the bottom, opening upwards. This cylinder is filled by a solid piston EF, covered externally with leather or tow, by which means it fits the box of the cylinder exactly, and allows no water to escape by its sides. There is a pipe KHD, which communicates laterally with this cylinder, and has a valve at some convenient place H, as near as possible to its junction with the cylinder. This valve also opens upwards. This pipe, usually called the RISING PIPE, or MAIN, terminates at the place D, where the water must be delivered.

Now suppose this apparatus set into the water, so that the upper end of the cylinder may be under or even with the surface of the water AB, the water will open the valve G, and after filling the barrel and lateral pipe, will also open the valve H, and at last stand at an equal height within and without. Now let the piston be put in at the top of the working barrel, and thrust down to K. It will push the water before it. This will shut the valve G, and the water will make its way through the valve H, and fill a part B *b* of the rising pipe, equal to the internal capacity of the working barrel. When this downward motion of the piston ceases, the valve H will fall down by its own weight and shut this passage. Now let the piston be drawn up again: the valve H hinders the water in the rising pipe from returning into the working barrel. But now the valve G is opened by the pressure of the external water, and the water enters and fills the cylinder as the piston rises. When the piston has got to the top, let it be thrust down again: the valve G will again be shut, and the water will be forced through the passage at H, and rise along the main, pushing before it the water already there, and will now have its surface at L. Repeating this operation, the water must at last arrive at D, however remote, and the next stroke would raise it to *e;* so that during the next rise of the piston the water in *e* D will be running off by the spout.

The effect will be the same whatever is the position of the working barrel, provided only that it be under water. It may lie horizontally or sloping, or it may be with its mouth and piston rod undermost. It is still the same forcing pump, and operates in the same manner and by the same means, viz. the pressure of the surrounding water.

The external force which must be applied to produce this effect is opposed by the pressure exerted by the water on the opposite face of the piston. It is evident, from the common laws of hydrostatics, that this opposing pressure is equal to the weight of a pillar of water, having the face of the piston for its base, and the perpendicular height *d* A of the place of delivery above the surface of the water AB in the cistern for its height. The form and dimensions of the rising pipe are indifferent in this respect, because heavy fluids press only in the proportion of their perpendicular height. Observe that it is not *d* F, but *d* A, which measures this pressure, which the moving force must balance and surmount. The whole pressure on the under surface F*f* of the piston is indeed equal to the weight of the pillar *d* F*f*δ; but part of this is balanced by the water AF*fa*. If indeed the water does not get into the upper part of the working barrel, this compensation does not obtain. While we draw up the piston, this pressure is removed, because all communication is cut off by the valve H, which now bears the whole pressure of the water in the main. Nay, the ascent of the piston is even assisted by the pressure of the surrounding water. It is only during the descent of the piston therefore that the external force is necessary.

Observe that the measure now given of the external force is only what is necessary for *balancing* the pressure of the water in the rising pipe. But in order that the pump may perform work, it must *surmount* this pressure, and cause the water to issue at D with such a velocity that the required quantity of water may be delivered in a given time. This requires force, even although here were no opposing pressure; which would be the case if the main were horizontal. The water fills it, but it is at rest. In order that a gallon, for instance, may be delivered in a second, the whole water in the horizontal main must be put in motion with a certain velocity. This requires force. We must therefore always distinguish between the state of equilibrium and the state of actual working. It is the equilibrium only that we consider at present; and no more is necessary for understanding the operation of the different species of pumps. The other force is of much more intricate investigation, and will be considered by itself.

The simplest form and situation of the lifting pump is represented by the sketch Fig. 3. The pump is immersed in the cistern till both the valve G and piston F are under the surface AB of the surrounding water. By this means the water enters the pump, opening both valves, and finally stands on a level within and without.

Now draw up the piston to the surface A. It must lift up the water which is above it (because the valve in the piston remains shut by its own weight); so that its surface will now be at *a*, A *a* being made equal to AF. In the meantime, the pressure of the surrounding water forces it into the working barrel, through the valve G; and the barrel is now filled with water. Now, let the piston be pushed down again; the valve G immediately shuts by its own weight, and in opposition to the endeavours which the water in the barrel makes to escape this way. This attempt to compress the water in the barrel causes it to open the valve F in the piston; or rather, this valve yields to our endeavour to push the piston down through the water in the working barrel. By this means we get the piston to the bottom of the barrel; and it has now above it the whole pillar of water reaching to the height *a*. Drawing up the piston to the surface A a second time, must lift this double column along with it, and its surface now will be at *b*. The piston may again be thrust down through the water in the barrel, and again drawn up to the surface, which will raise the water to *c*. Another repetition will raise it to *d*; and it will now show itself at the intended place of delivery. Another repetition will raise it to *e*; and while the piston is now descending to make another stroke, the water in *e d* will be running off through the spout D; and thus a stream will be produced, in some degree continual, but very unequal. This is inconvenient in many cases: thus, in a pump for domestic uses, such a hobbling stream would make it very troublesome to fill a bucket. It is therefore usual to terminate the main by a cistern LMNO, and to make the spout small. By this means the water brought up by the successive strokes of the piston rises to such a height in this cistern, as to produce an efflux by the spout nearly equable. The smaller we make the spout D the more equable will be the stream; for when the piston brings up more water than can be discharged during its descent, some of it remains in the cistern. This, added to the supply of next stroke, makes the water rise higher in the cistern than it did by the preceding stroke. This will cause the efflux to be quicker during the descent of the piston, but perhaps not yet sufficiently quick to discharge the whole supply. It therefore rises higher next stroke; and at last it rises so high, that the increased velocity of efflux makes the discharge precisely balance the supply. Now, the quantity supplied in each stroke is the same, and occupies the same room in the cistern at top: and the surface will sink the same number of inches during the descent of the piston, whether that surface has been high or low at the beginning. But because the velocities of the efflux are as the square roots of the heights of the water above the spout, it is evident that a sink of two or three inches will make a smaller change in the velocity of efflux when this height and velocity are great. This seems but a trifling observation; but it serves to illustrate a thing to be considered afterwards, which is important and abstruse, but perfectly similar to this.

It is evident, that the force necessary for this operation must be equal to the weight of the pillar of water *d* A *a* D, if the pipe be perpendioular. If the pump be standing aslope, the pressure which is to be balanced is still equal to the weight of a pillar of water of this perpendicular height, and having the surface of the piston for its base.

Such is the simpiest, and, we may add, by far the best, form of the forcing and lifting pumps; but it is not the most usual. Cirucmstances of convenience, economy, and more frequently of fancy and habit, have caused the pump-makers to deviate greatly from this form. It is not usual to have the working-barrel in the water; this, especially in deep wells, makes it of difficult access for repairs, and requires long piston-rods. This would not do in a forcing pump, because they would bend.

We have supposed, in our account of the lifting-pump, that the rise of the piston always terminated at the surface of the water in the cistern. This we did in order that the barrel might always be filled by the pressure of the surrounding water. But let us suppose that the rise of the piston does not end here, and that it is gradually drawn up to the very top: it is plain that the pressure of the atmosphere is by this means taken off from the water in the pipe (see PNEUMATICS), while it remains pressing on the water of the cistern. It will therefore cause the water to follow the piston as it rises through the pipe, and it will raise it in this way 33 feet at a medium. If, therefore, the spout D is not more than 33 feet above the surface of the water in the cistern, the pipe will be full of water when the piston is at D. Let it be pushed down to the bottom; the water will remain in the pipe, because the valve G will shut: and thus we may give the piston a stroke of any length not exceeding 33 feet. If we raise it higher than this, the water will not follow; but it will remain in the pipe, to be lifted by the piston, after it has been pushed down through it to the bottom.

But it is not necessary, and would be very inconvenient, to give the piston so long a stroke. The great use of a pump is to render effectual the reciprocation of a short stroke which we can command, while such a long stroke is generally out of our power. Suppose that the piston is pushed down only to *b*, it will then have a column *bf* incumbent on it, and it will lift this column when again drawn up. And this operation may be repeated like the former, when the piston was always under water; for the pressure of the atmosphere will always cause the water to follow the piston to the height of 33 feet.

Nor is it necessary that the fixed valve G be placed at the lower orifice of the pipe, nor even under water. For, while things are in the state now described, the piston drawn up to *f*, and the whole pipe full of water; if we suppose another valve placed at *b* above the surface of the cistern, this valve can do no harm. Now let the piston descend, both valves G and *b* will shut. G may now be removed, and the water will remain supported in the space *b* G by the air; and now the alternate motions of the piston will produce the same effect as before.

We found in the former case that the piston was carrying a load equal to the weight of a pillar of water of the height AD, because the surrounding water could only support it at its own level. Let us see what change is produced by the assistance of the pressure of the atmosphere. Let the under surface of the piston be at *b*; when the piston was at *f*, 33 feet above the surface of the cistern, the water was raised to that height by the pressure of the atmosphere. Suppose a partition made at *b* by a thin plate, and all the water above it taken away. Now pierce a hole in this plate. The pressure of the atmosphere was able to carry the whole column *f a*. Part of this column is now removed, and the remainder is not a balance for the air's pressure. This will therefore cause the water to spout up through this hole and rise to *f*. Therefore the under surface of this plate is pressed up by the contiguous water with a force equal to the weight of that pillar of water which it formerly supported; that is, with a force equal to the weight of the pillar *f b*. Now, the under surface of the piston, when at *b*, is in the same situation. It is pressed upwards by the water below it, with a force equal to the weight of the column *f b*: but it is pressed downwards by the whole pressure of the atmosphere, which presses on all bodies; that is, with the weight of the pillar *f a*. On the whole, therefore, it is pressed downwards by a force equal to the difference of the weights of the pillars *f a* and *f b*; that is, by a force equal to the weight of the pillar *b a*.

It may be conceived better perhaps in this way. When the piston was under the surface of the water in the cistern, it was equally pressed on both sides, both by the water and atmosphere. The atmosphere exerted its pressure on it by the intervention of the water; which being, to all sense, a perfect fluid, propagates every external pressure undiminished. When the piston is drawn up above the surface of the pit-water, the atmosphere continues to press on its upper surface with its whole weight, through the intervention of the water which lies above it; and its pressure must therefore be added to that of the incumbent water. It also continues to press on the under surface of the piston by the intervention of the water; that is, it presses this water to the piston. But, in doing this, it carries the weight of this water which it is pressing on the piston. The pressure on the piston therefore is only the excess of the whole pressure of the atmosphere above the weight of the column of water which it is supporting. Therefore the difference of atmospheric pressure on the upper and under surfaces of the piston is precisely equal to the weight of the column of water supported in the pipe by the air. It is not, however, the individual weight of this column that loads the piston; it is the part of the pressure of the atmosphere on its upper surface, which is not balanced by its pressure on the under surface.

In attempting therefore to draw up the piston, we have to surmount this unbalanced part of the pressure of the atmosphere, and also the weight of the water which lies above the piston, and must be lifted by it: and thus the whole opposing pressure is the same as before, namely, the weight of the whole vertical pillar reaching from the surface of the water in the cistern to the place of delivery. Part of this weight is immediately carried by the pressure of the atmosphere; but, in lieu of it, there is an equal part of this pressure of the atmosphere abstracted from the under surface of the piston, while its upper surface sustains its whole pressure.

So far, then, these two states of the pump agree.—But they differ exceedingly in their mode of operation; and there are some circumstances not very obvious which must be attended to, in order that the pump may deliver any water at the spout D. This requires, therefore, a serious examination.

Let the fixed valve G (Fig. 4.) be supposed at the surface of the cistern water. Let M *m* be the lowest, and N *n* the highest, positions of the piston, and let HA=*h* be the height of a column of water equiponderant with the atmosphere.

When the pump is filled, not with water but with air, and the piston is in its lowest position, and all in equilibrio, the internal air has the same density and elasticity with the external. The space MA *a m*, therefore, contains air of the common density and elasticity. These may be measured by *h*, or the weight of a column of water whose height is *h*. Now, let the piston be drawn up to N *n*. The air which occupied the space MA *a m* now occupies the space NA *a n*, and its density is now $\frac{\mathrm{MA}\,a\,m}{\mathrm{NA}\,a\,n}$. Its elasticity is now diminished, being proportionable to its density (see PNEUMATICS), and no longer balances the pressure of the atmosphere. The valve G will therefore be forced up by the water, which will rise to some height SA. Now let the piston again descend to M *m*. It cannot do this with its valve shut; for, when it comes down so far as to reduce the air again to its common density, it is not yet at M, because the space below it has been diminished by the water which got into the pipe, and is retained there by the valve G. The piston valve therefore opens by the air which we thus attempt to compress, and the superfluous air escapes. When the piston has got to M, the air is again of the common density, and occupies the space MS*s m*. Now draw the piston up to N. This air will expand into the space NS *s n*, and its density will be reduced to $\frac{\mathrm{MS}\,s\,m}{\mathrm{NS}\,s\,n}$, and its elasticity will no longer balance the pressure of the atmosphere, and more water will enter, and it will rise higher. This will go on continually. But it may happen that the water will never rise so high as to reach the piston, even though not 33 feet above the water in the cistern: for the successive diminutions of density and elasticity are a series of quantities that decrease geometrically, and therefore will have a limit. Let us see what determines this limit.

At whatever height the water stands in the lower part of the pipe, the weight of the column of water $SAas$, together with the remaining elasticity of the air above it, exactly balances the pressure of the atmosphere (see PNEUMATICS). Now the elasticity of the air in the space $NSsn$ is equal to $h \times \frac{MS\,sm}{NS\,sn}$. Therefore, in the case where the limit obtains, and the water rises no farther, we must have $h = AS + h\frac{MS\,sm}{NS\,sn}$, or because the column is of the same diameter throughout, $h = AS + h\,\frac{MS}{NS}$ and $\frac{MS}{NS}\,h = h - AS, = HS$, and $NS : MS = HA : HS$, and $NS - MS : NS = HA - HS : HA$, or $NM : NS = AS : AH$, and $NM \times AH = NS \times AS$.—Therefore, if AN, the distance of the piston in its highest position from the water in the cistern, and NM the length of its stroke, be given, there is a certain determined height AS to which the water can be raised by the pressure of the air: for AH is a constant quantity; and therefore when MN is given, the rectangle AS×SN is given. If this height AS be less than that of the piston in its lowest position, the pump will raise no water, although AN may be less than AH. Yet the same pump will raise water very effectually, if it be first of all filled with water; and we have seen professional engineers much puzzled by this capricious failure of their pumps. A little knowledge of the principles would have prevented their disappointment.

To insure the delivery of water by the pump, the stroke must be such that the rectangle MN×AH may be greater than any rectangle that can be made of the parts AN, that is, greater than the square of half AN. Or, if the length of the stroke be already fixed by other circumstances, which is a common case, we must make AN so short that the square of its half, measured in feet, shall be less than 33 times the stroke of the piston.

Suppose that the fixed valve, instead of being at the surface of the water in the cistern, is at S, or anywhere between S and A, the performance of the pump will be the same as before: but if it be placed anywhere above S, it will be very different. Let it be at T. It is plain that when the piston is pushed down from N to M, the valve at T prevents any air from getting down; and therefore, when the piston is drawn up again, the air contained in the space MT$t\,m$ will expand into the space NT$t\,n$, and its density will be $\frac{\text{MT}}{\text{NT}}$. This is less than $\frac{\text{MS}}{\text{NS}}$, which expresses the density of the air which was left in the space TS$s\,t$ by the former operations.—The air, therefore, in TS$s\,t$ will also expand, will open the valve, and now the water will rise above S. The proportion of NS to NT may evidently be such that the water will even get above the valve T. This diminishes the space NT$t\,n$; and therefore, when the piston has been pushed down to M, and again drawn up to N, the air will be still more rarefied, and the water will rise still higher. The foregoing reasoning, however, is sufficient to show that there may still be a height which the water will not pass, and that this height depends on the proportion between the stroke of the piston and its distance from the water in the cistern. We need not give the determination, because it will come in afterwards in combination with other circumstances. It is enough that the reader sees the physical causes of this limitation: and, lastly, we see plainly that the utmost security will be given for the performance of the pump, when the fixed valve is so placed that the piston, when in its lowest position, shall come into contact with it. In this case, the rarefaction of the air will be the completest possible; and if there were no space left between the piston and valve, and all were perfectly air-tight, the rarefaction would be complete, and the valve might be any thing less than 33 feet from the surface of the water in the cistern.

But this perfect contact and tightness is unattainable; and though the pump may be full of water, its continual downward pressure causes it to filtrate slowly through every crevice, and the air enters through every pore, and even disengages itself from the water, with which a considerable portion had been chemically combined. The pump by this means loses water, and it requires several strokes of brisk working to fill it again: and if the leathers have become dry, so much admission may be given to the air, that the pump will not fill itself with water by *any working*. It is then necessary to pour water into it, which shuts up these passages, and soon sets all to rights again. For these reasons, it is always prudent to place the fixed valve as low as other circumstances will permit, and to make the piston-rod of such a length, that when it is at the bottom of its stroke it shall be almost in contact with the valve. When we are not limited by other circumstances, it is evident that the best possible form is to have both the piston and the fixed valve under the surface of the water of the cistern. In this situation they are always wet and air-tight. The chief objection is, that by this disposition they are not easily come at when needing repair. This is a material objection in deep mines. In such situations, therefore, we must make the best compensation of different circumstances that we can. It is usual to place the fixed valve at a moderate distance from the surface of the water, and to have a hole in the side of the pipe, by which it may be got out. This is carefully shut up by a plate firmly screwed on, with leather or cement between the parts. This is called the *clack door*. It would, in every case, be very proper to have a fixed valve in the lower end of the pipe. This would combine all advantages. Being always tight, the pipe would retain the water, and it would leave to the valve above it its full effect of increasing the rarefaction. A similar hole is made in the working barrel, a little above the highest position of the piston. When this needs repair, it can be got at through this hole, without the immense trouble of drawing up the whole rods.

Thus we have conducted the reader, step by step, from the simplest form of the pump to that which long experience has at last selected as the most generally convenient. This we shall now describe in some detail.

The *Sucking-Pump* consists of two pipes DCCD, BAAB (Fig. 5.), of which the former is called the *Barrel*, or the *Working-Barrel*, and the other is called the *Suction-Pipe*, and is commonly of a smaller diameter.—These are joined by means of stanches E, F, pierced with holes to receive screwed bolts. A ring of leather, or of lead, covered with a proper cement, is put between them; which, being strongly compressed by the screw-bolts, renders the joint perfectly air-tight. The lower end A of the suction-pipe is commonly spread out a little to facilitate the entry of the water, and frequently has a grating across it at AA to keep out filth or gravel. This is immerged in the standing water YZ. The working-barrel is cylindrical, as evenly and smoothly bored as possible, that the piston may fill it exactly through its whole length, and move along it with as little friction as may be consistent with air-tightness.

The piston is a sort of truncated cone OPKL, generally made of wood not apt to split, such as elm or beech. The small end of it is cut off at the sides, so as to form a sort of arch OQP, by which it is fastened to the iron rod or spear. It is exhibited in different positions in Figs. 6, 7, which will give a more distinct notion of it than any description. The two ends of the conical part may be hooped with brass. This cone has its larger end surrounded with a ring or band of strong leather fastened with nails, or by a copper hoop, which is driven on it at the smaller end.

This band should reach to some distance beyond the base of the cone; the farther the better: and the whole must be of uniform thickness all round, so as to suffer equal compression between the cone and the working-barrel. The seam or joint of the two ends of this band must be made very close, but not sewed or stitched together. This would occasion bumps or inequalities, which would spoil its tightness; and no harm can result from the want of it, because the two edges will be squeezed close together by the compression in the barrel. It is by no means necessary that this compression be great. This is a very detrimental error of the pump-makers. It occasions enormous friction, and destroys the very purpose which they have in view, viz. rendering the piston air-tight; for it causes the leather to wear through very soon at the edge of the cone, and it also wears the working-barrel. This very soon becomes wide in that part which is continually passed over by the piston, while the mouth remains of its original diameter, and it becomes impossible to thrust in a piston which shall completely fill the worn part. Now, a very moderate pressure is sufficient for rendering the pump perfectly tight, and a piece of glove leather would be sufficient for this purpose, if loose or detached from the solid cone; for suppose such a loose and flexible, but impervious, band of leather put round the piston, and put into the barrel; and let it even be supposed that the cone does not compress it in the smallest degree to its internal surface.—Pour a little water carefully into the inside of this sort of cup or dish, it will cause it to swell out a little, and apply itself close to the barrel all round, and even adjust itself to all its inequalities. Let us suppose it to touch the barrel in a ring of an inch broad all round. We can easily compute the force with which it is pressed. It is half the weight of a ring of water an inch deep and an inch broad. This is a trifle, and the friction occasioned by it not worth regarding; yet this trifling pressure is sufficient to make the passage perfectly impervious, even by the most enormous pressure of a high column of incumbent water: for let this pressure be ever so great, the pressure by which the leather adheres to the barrel always exceeds it, because the incumbent fluid has no *preponderating* power by which it can force its way between them, and it must insinuate itself precisely so far, that its pressure on the inside of the leather shall still exceed, and only exceed, the pressure by which it endeavours to insinuate itself; and thus the piston becomes perfectly tight with the smallest possible friction. This reasoning is perhaps too refined for the uninstructed artist, and probably will not persuade him. To such we would recommend an examination of the pistons and valves contrived and executed by that artist, whose skill far surpasses our highest conceptions, the all-wise Creator of this world. The valves which shut up the passages of the veins, and this in places where an extravasation would be followed by instant death, are cups of thin membrane, which adhere to the sides of the channel about half way round, and are detached in the rest of their circumference. When the blood comes in the opposite direction, it pushes the membrane aside, and has a passage perfectly free. But a stagnation of motion allows the tone of the muscular (perhaps) membrane, to restore it to its natural shape, and the least *motion* in the opposite direction causes it instantly to clap close to the sides of the vein, and then no pressure whatever can force a passage. We shall recur to this again, when describing the various contrivances of valves, &c. What we have said is enough for supporting our directions for constructing a tight piston. But we recommended thick and strong leather, while our present reasoning seems to render thin leather preferable. If the leather be thin, and the solid piston in any part does not press it gently to the barrel, there will be in this part an unbalanced pressure of the incumbent column of water, which would instantly burst even a strong leather bag; but when the solid piston, covered with leather, exactly fills the barrel, and is even pressed a little to it, there is no such risk; and now that part of the leather-band which reaches beyond the solid piston performs its office in the completest manner. We do not hesitate, therefore, to recommend this form of a piston, which is the most common and simple of all, as preferable, when well executed, to any of those more artificial, and frequently very ingenious, constructions, which we have met with in the works of the first engineers. To proceed, then, with our description of the sucking-pump.

At the joining of the working-barrel with the suction-pipe, there is a hole H, covered with a valve opening upwards. This hole H is either made in a plate which makes a part of the suction-pipe, being cast along with it, or it is made in a separate plate. This last is the most convenient, being easily removed and replaced. Different views are given of this valve in Fig. 8, 9, 10. The diameter EF (Fig. 10.) of this plate is the same with that of the flanches, and it has holes corresponding to them, through which their bolts pass which keep all together. A ring of thick leather NKL is applied to this plate, having a part cut out between N and L, to make room for another piece of strong leather NR (Fig. 9.) which composes the valve. The circular part of this valve is broader than the hole in the middle of Fig. 10. but not quite so broad as to fill up the inside of the ring of leather OQP of this Fig. which is the same with GKI of Fig 10. The middle of this leather valve is strengthened by two brass (not iron) plates, the uppermost of which is seen at R of Fig. 9: the one on its under side is a little smaller than the hole in the valve-plate, that it may go freely in; and the upper plate R is larger than this hole, that it may compress the leather to its brim all round. It is evident, that when this plate with its leathers is put between the joint flanches, and all is screwed together, the tail of leather N of Fig. 9. will be compressed between the plates, and form a hinge, on which the valve can turn, rising and falling. There is a similar valve fastened to the upper side, or broadest base of the piston. This description serves for both valves, and in general for most valves which are to be found in any parts of a pump.

The reader will now understand, without any repetition, the process of the whole operation of a sucking-pump. The piston rarefies the air in the working-barrel; and that in the suction-pipe expands through the valve into the barrel; and being no longer a balance for the atmospheric pressure, the water rises into the suction-pipe; another stroke of the piston produces a similar effect, and the water rises farther, but by a smaller step than by the preceding stroke; by repeating the strokes of the piston, the water gets into the barrel; and when the piston is now pushed down through it, it gets above the piston, and must now be lifted up to any height. The suction-pipe is commonly of smaller size than the working-barrel, for the sake of economy. It is not necessary that it be so wide: but it may be, and often is, made too small. It should be of such a size, that the pressure of the atmosphere may be able to fill the barrel with water as fast as the piston rises. If a void is left below the piston, it is evident that the piston must be carrying the whole weight of the atmosphere, besides the water which is lying above it. Nay, if the pipe be only so wide, that the barrel shall fill precisely as fast as the piston rises, it must sustain all this pressure. The suction-pipe should be wider than this, that all the pressure of the atmosphere which exceeds the weight of the pillar in the suction-pipe may be employed in pressing it on the under surface of the piston, and thus diminish the load. It cannot be made too wide; and too strict an economy in this respect may very sensibly diminish the performance of the pump, and more than defeat its own purpose. This is most likely when the suction-pipe is long, because there the length of the pillar of water nearly balances the air's pressure, and leaves very little accelerating force; so that water will rise but slowly even in the widest pipe. All these things will be made the subjects of computation afterwards.

It is plain that there will be limitations to the rise of the water in the suction-pipe, similar to what we found when the whole pump was an uniform cylinder. Let a be the height of the fixed valve above the water in the cistern: let B and b be the spaces in cubic measure between this valve and the piston in its highest and lowest positions, and therefore express the bulks of the air which may occupy these spaces: let y be the distance between the fixed valve and the water in the suction-pipe, when it has attained its greatest height by the rarefaction of the air above it: let h be the height of a column of water in equilibrio, with the whole pressure of the atmosphere, and therefore having its weight in equilibrio with the elasticity of common air: and let x be the height of the column whose weight balances the elasticity of the air in the suction-pipe, when rarefied as much as it can be by the action of the piston, the water standing at the height $a - y$.

Then, because this elasticity, together with the column $a - y$ in the suction-pipe, must balance the whole pressure of the atmosphere, (see PNEUMATICS), we must have $h = x + a - y$, and $y = a + x - h$.

When the piston was in its lowest position, the bulk of the air between it and the fixed valve was b. Suppose the valve kept shut, and the piston raised to its highest position, the bulk will be B, and its density $\frac{b}{B}$, and its elasticity, or the height of the column whose weight will balance it, will be $h\,\frac{b}{B}$. If the air in the suction-pipe be denser than this, and consequently more elastic, it will lift the valve, and some will come in; therefore, when the pump has rarefied the air as much as it can, so that none does, in fact, come in, the elasticity of the air in the suction-pipe *must* be the same. Therefore $x = h\frac{b}{B}$.

We had $y = a + x - h$. Therefore $y = a + h\frac{b}{B} - h, = a + \frac{b - B}{B}h, = a - \frac{B - b}{B}h$.

Therefore when $\frac{B - b}{B}h$ is less than a, the water will stop before it reaches the fixed valve. But when a is less than $\frac{B - b}{B}h$, the water will get above the fixed valve, y becoming negative.

But it does not follow that the water will reach the piston, that is, will rise so high that the piston will pass through it in its descent. Things now come into the condition of a pump of uniform dimensions from top to bottom; and this point will be determined by what was said when treating of such a pump.

There is another form of the sucking pump which is much used in great water-works, and is of equal efficacy with the one now described. It is indeed the same pump in an inverted position. It is represented in Fig. 11. where ABCD is the working-barrel, immersed, with its mouth downwards, in the water of the cistern. It is joined by means of flanches to the rising pipe or MAIN.

This usually consists of two parts. The first, BEFC, is bent to one side, that it may give room for the iron frames TXYV, which carries the rod NO of the piston M, attached to the traverses RS, TOV of this frame. The other part, EGHF, is usually of a less diameter, and is continued to the place of delivery. The piston frame XTVY hangs by the rod Z, at the arm of a lever or working beam, not brought into the figure. The piston is perforated like the former, and is surrounded like it with a band of leather in form of a taper-dish. It has a valve

K, on its broad or upper base, opening when pushed from below. The upper end of the working-barrel is pierced with a hole, covered with a valve I, also opening upwards.

Now suppose this apparatus immersed into the cistern till the water is above it, as marked by the line 2, 3, and the piston drawn up till it touch the end of the barrel. When the piston is allowed to descend by its own weight, the water rises up through its valve K, and fills the barrel. If the piston be now drawn up by the moving power of the machinery with which it is connected, the valve K shuts, and the piston pushes the water before it through the valve I into the main pipe EFGH. When the piston is again let down, the valve I shuts by its own weight and the pressure of the water incumbent on it, and the barrel is again filled by the water of the cistern. Drawing up the piston pushes this water into the main pipe, &c. and thus the water is at length delivered at the place required.

This pump is usually called the *Lifting-pump*; perhaps the simplest of all in its principle and operation. It needs no farther explanation: and we proceed to describe

The FORCING-PUMP, represented in Fig. 12. It consists of a working barrel ABCD, a suction-pipe CDEF, and a main or rising pipe. This last is usually in three joints. The first GHKI may be considered as making part of the working barrel, and is commonly cast in one piece with it. The second IKLM is joined to it by flanches, and forms the elbow which this pipe must generally have. The third LNOM is properly the beginning of the main, and is continued to the place of delivery. At the joint IK there is a hanging valve or clack S; and there is a valve R on the top of the suction-pipe.

The piston PQTV is solid, and is fastened to a stout iron rod which goes through it, and is fixed by a key drawn through its end. The body of the piston is a sort of double cone, widening from the middle to each end, and is covered with two bands of very strong leather, fitted to it in the manner already described.

The operation of this pump is abundantly simple.—When the piston is thrust into the pump, it pushes the air before it through the valve S, for the valve R remains shut by its own weight. When it has reached near the bottom, and is drawn up again, the air which filled the small space between the piston and the valve S now expands into the barrel; for as soon as the air begins to expand, it ceases to balance the pressure of the atmosphere, which therefore shuts the valve S. By the expansion of the air in the barrel the equilibrium at the valve R is destroyed, and the air in the suction-pipe lifts the valve, and expands into the barrel; consequently it ceases to be a balance for the pressure of the atmosphere, and the water is forced into the suction-pipe. Pushing the piston down again forces the air into the barrel through the valve S, the valve R in the meantime shutting. When the piston is again drawn up, S shuts, R opens, the air in the suction-pipe dilates anew, and the water rises higher in it. Repeating these operations, the water gets at last into the working barrel, and is forced into the main by pushing down the piston, and is pushed along to the place of delivery.

The operation of this pump is therefore twofold, sucking and forcing. In the first operation, the same force must be employed as in the sucking-pump, namely, a force equal to the weight of a column of water having the section of the piston for its base, and the height of the piston above the water in the cistern for its height. It is for the sake of this part of the operation that the upper cone is added to the piston. The air and water would pass by the sides of the lower cone while the piston is drawn up; but the leather of the upper cone applies to the surface of the barrel, and prevents this. The space contained between the barrel and the valve S is a great obstruction to this part of the operation, because this air cannot be rarefied to a very great degree. For this reason, the suction-pipe of a forcing-pump must not be made long. It is not indeed necessary; for by placing the pump a few feet lower, the water will rise into it without difficulty, and the labour of suction is as much diminished as that of impulsion is increased. However, an intelligent artist will always endeavour to make this space between the valve S and the lowest place of the piston as small as possible.

The power employed in forcing must evidently surmount the pressure of the whole water in the rising pipe, and (independent of what is necessary for giving the water the required velocity, so that the proper quantity per hour may be delivered) the piston has to withstand a force equal to the weight of a column of water having the section of the piston for its base, and the perpendicular altitude of the place of delivery above the lower surface of the piston for its height. It is quite indifferent in this respect what is the diameter of the rising pipe; because the pressure on the piston depends on the altitude of the water only, independent of its quantity. We shall even see that a small rising pipe will require a greater force to convey the water along it to any given height or distance.

When we would employ a pump to raise water in a crooked pipe, or in any pipe of moderate dimensions, this form of pump, or something equivalent, must be used. In bringing up great quantities of water from mines, the common sucking-pump is generally employed, as really the best of them all: but it is the most expensive, because it requires the pipe to be perpendicular, straight, and of great dimensions, that it may contain the piston-rods. But this is impracticable when the pipe is crooked.

If the forcing-pump, constructed in the manner now described, be employed, we cannot use forcers with long rods. These would bend when pushed down by their further extremity. In this case, it is usual to employ only a short and stiff rod, and to hang it by a chain, and load it with a weight superior to the weight of water to be raised by it. The machinery therefore is employed, not in forcing the water along the rising pipe, but in raising the weight which is to produce this effect by its subsequent descent.

In this case, it would be much better to employ the lifting-pump of Fig. 11. For as the load on the forcers must be greater than the resistances which it must surmount, the force exerted by the machine must in like manner be greater than this load. This double excess would be avoided by using the lifting-pump.

It will readily occur to the reader that the quantity of water delivered by any pump will be in the joint proportion of the surface or base of the piston and its velocity; for this measures the capacity of that part of the working-barrel which the piston passes over. The velocity of the water in the conduit-pipe, and in its passage through every valve, will be greater or less than the velocity of the piston in the same proportion that the area of the piston or working-barrel is greater or less than the area of the conduit or valve. For whatever quantity of water passes through any section of the working-barrel in a second, the same quantity must go through any one of these passages. This enables us to modify the velocity of the water as we please: we can increase it to any degree at the place of delivery by diminishing the aperture through which it passes, provided we apply sufficient force to the piston.

It is evident that the operation of a pump is by starts, and that the water in the main remains at rest, pressing on the valve during the time that the piston is withdrawn from the bottom of the working-barrel. It is in most cases desirable to have this motion equable, and in some cases it is absolutely necessary. Thus, in the engine for extinguishing fires, the spout of water going by jerks could never be directed with a certain aim, and half of the water would be lost by the way; because a body at rest cannot in an instant be put in rapid motion, and the first portion of every jerk of water would have but a small velocity. A very ingenious contrivance has been fallen upon for obviating this inconvenience, and procuring a stream nearly equable. We have not been able to discover the author. At any convenient part of the rising pipe beyond the valve S there is annexed a capacious vessel VZ (Fig. 13. No 1 and 2.) close atop, and of great strength. When the water is forced along this pipe, part of it gets into this vessel, keeping the air confined above it, and it fills it to such a height V, that the elasticity of the confined air balances a column reaching to T, we shall suppose, in the rising pipe. The next stroke of the piston sends forward more water, which would fill the rising pipe to some height above T. But the pressure of this additional column causes some more of it to go into the air vessel, and compress its air so much more that its elasticity now balances a longer column. Every succeeding stroke of the piston produces a like effect. The water rises higher in the main pipe, but some more of it goes into the air-vessel. At last the water *appears at the* place of delivery; and the air in the air-vessel is now so much compressed that its elasticity balances the pressure of the whole column. The next stroke of the piston sends forward some more water. If the diameter of the orifice of the main be sufficient to let the water flow out with a velocity equal to that of the piston, it will so flow out, rising no higher, and producing no sensible addition to the compression in the air-vessel. But if the orifice of the main be contracted to half its dimensions, the water sent forward by the piston cannot flow out in the time of the stroke without a greater velocity, and therefore a greater force. Part of it, therefore, goes into the air-vessel, and increases the compression. When the piston has ended its stroke, and no more water comes forward, the compression of the air in the air-vessel being greater than what was sufficient to balance the pressure of the water in the main pipe, now forces out some of the water which is lying below it. This cannot return towards the pump, because the valve S is now shut. It therefore goes forward along the main, and produces an efflux during the time of the piston's rising in order to make another stroke. In order that this efflux may be very equable, the air-vessel must be very large. If it be small, the quantity of water that is discharged by it during the return of the piston makes so great a portion of its capacity, that the elasticity of the confined air is too much diminished by this enlargement of its bulk, and the rate of efflux must diminish accordingly. The capacity of the air-vessel should be so great that the change of bulk of the compressed air during the inaction of the piston may be inconsiderable. It must therefore be very strong.

It is pretty indifferent in what way this air-vessel is connected with the rising pipe. It may join it laterally, as in Fig. 13, No 1. and the main pipe go on without interruption; or it may be made to *surround* an interruption of the main pipe, as in Fig. 13, No 2. It may also be in any part of the main pipe. If the sole effect intended by it is to produce an equable jet, as in ornamental water-works, it may be near the end of the main. This will require much less strength, because there remains but a short column of water to compress the air in it. But it is, on the whole, more advantageous to place it as near the pump as possible, that it may produce an equable motion in the whole main pipe. This is of considerable advantage; when a column of water several hundred feet long is at rest in the main pipe, and the piston at one end of it put at once into motion, even with a moderate velocity, the strain on the pipe would be very great. Indeed if it were possible to put the piston instantaneously into motion with a finite velocity, the strain on the pipe, tending to burst it, would be next to infinite. But this seems impossible in nature; all changes of motion *which we observe* are gradual, because all impelling bodies have some elasticity or softness by which they yield to compression. And, in the way in which pistons are commonly moved, *viz.* by cranks, or something analogous to them, the motion is *very sensibly* gradual. But still the air-vessel tends to make the motion along the main-pipe less desultory, and therefore diminishes those strains which would really take place in the main pipe. It acts like the springs of a travelling-carriage, whose jolts are incomparably less than those of a cart; and by this means really enables a given force to propel a greater quantity of water in the same time.

We may here by the way observe, that the attempts of mechanicians to correct this unequal motion of the piston-rod are misplaced, and if it could be done, would greatly hurt a pump. One of the best methods of producing this effect is to make the piston-rod consist of two parallel bars, having teeth in the sides which front each other. Let a toothed wheel be placed between them, having only the half of its circumference furnished with teeth. It is evident, without any farther description, that if this wheel be turned uniformly round its axis, the piston-rod will be moved uniformly up and down without intermission. This has often been put in practice; but the machine always went by jolts, and seldom lasted a few days. Unskilled mechanicians attributed this to defect in the execution: but the fault is essential, and lies in the principle.

The machine could not perform one stroke, if the first mover did not slacken a little, or the different parts of the machine did not yield by bending or by compression; and no strength of materials could withstand the violence of the strains at every reciprocation of the motion. This is chiefly experienced in great works which are put in motion by a water-wheel, or some other equal power exerted on the mass of matter of which the machine consists. The water-wheel being of great weight, moves with considerable steadiness or uniformity; and when an additional resistance is opposed to it by the beginning of a new stroke of the piston, its great quantity of motion is but little affected by this addition, and it proceeds very little retarded; and the machine must either yield a little by bending and compression, or go to pieces, which is the common event. Cranks are free from this inconvenience, because they accelerate the piston gradually, and bring it gradually to rest, while the water-wheel moves round with almost perfect uniformity. The only inconvenience (and it may be considerable) attending this slow motion of the piston at the beginning of its stroke is, that the valves do not shut with rapidity, so that some water gets back through them. But when they are properly formed and loaded, this is but trifling.

We must not imagine, that because the stream produced by the assistance of an air-barrel is almost perfectly equable, and because as much water runs out during the returning of the piston as during its active stroke, it therefore doubles the quantity of water. No more water can run out than what is sent forward by the piston during its effective stroke. The continued stream is produced only by preventing the whole of this water from being discharged during this time, and by providing a propelling force to act during the piston's return. Nor does it enable the moving force of the piston to produce a double effect: for the compression which is produced in the air-vessel, more than what is necessary for merely balancing the quiescent column of water, reacts on the piston, resisting its compression just as much as the column of water would do which produces a velocity equal to that of the efflux. Thus if the water is made to spout with the velocity of eight feet per second, this would require an additional column of one foot high, and this would just balance the compression in the air-vessel, which maintains this velocity during the non-action of the piston. It is, however, a matter of fact, that a pump furnished with an air-vessel delivers a little more water than it would do without it. But the difference depends on the combination of many very dissimilar circumstances, which it is extremely difficult to bring into calculation. Some of these will be mentioned afterwards.

To describe, or even to enumerate, the immense variety of combinations of these three simple pumps would fill a volume. We shall select a few, which are more deserving of notice.

I. The common sucking-pump may, by a small addition, be converted into a lifting-pump, fitted for propelling the water to any distance and with any velocity.

Fig. 14. is a Sucking-pump, whose working-barrel ACDB has a lateral pipe AEGHF connected with it close to the top. This terminates in a *main* or rising pipe IK, furnished or not with a valve L. The top of the barrel is shut up by a strong plate MN, having a hollow neck terminating in a small flanch. The piston rod QR passes through this neck, and is nicely turned and polished. A number of rings of leather are put over the rod, and strongly compressed round it by another flanch and several screwed bolts, as is represented at OP. By this contrivance the rod is closely grasped by the leathers, but may be easily drawn up and down, while all passage of air or water is effectually prevented.

The piston S is perforated, and furnished with a valve opening upwards. There is also a valve T on the top of the suction-pipe YX; and it will be of advantage, though not absolutely necessary, to put a valve L at the bottom of the rising pipe. Now suppose the piston at the bottom of the working-barrel. When it is drawn up, it tends to compress the air above it, because the valve in the piston remains shut by its own weight. The air therefore is driven through the valve L into the rising pipe, and escapes. In the meantime, the air which occupied the small space between the piston and the valve T expands into the upper part of the working-barrel; and its elasticity is so much diminished thereby, that the atmosphere presses the water of the cistern into the suction-pipe, where it will rise till an equilibrium is again produced. The next downward stroke of the piston allows the air, which had come from the suction-pipe into the barrel during the ascent of the piston, to get through its valve. Upon drawing up the piston, this air is also drawn off through the rising-pipe. Repeating this process brings the water at last into the working-barrel, and it is then driven along the rising-pipe by the piston.

This is one of the best forms of a pump. The rarefaction may be very perfect, because the piston can be brought so near to the bottom of the working-barrel; and, for forcing water in opposition to great pressures, it appears preferable to the common forcing-pump; because in that the piston-rods are compressed and exposed to bending, which greatly hurts the pump by wearing the piston and barrel on one side. This soon renders it less tight, and much water squirts out by the sides of the piston. But in this pump the piston-rod is always drawn or pulled, which keeps it straight; and rods exert a much greater force in opposition to a pull than in opposition to compression. The collar of leather round the piston-rods is found by experience to need very little repairs, and is very impervious to water. The whole is very accessible for repairs; and in this respect much preferable to the common pump in deep mines, where every fault of the piston obliges us to draw up some hundred feet of piston-rods. By this addition, too, any common pump for the service of a house is converted into an engine for extinguishing fire, or may be made to convey the water to every part of the house; and this without hurting or obstructing its common uses. All that is necessary is to have a large cock on the upper part of the working-barrel opposite to the lateral pipe in this figure. This cock serves for a spout when the pump is used for common purposes; and the merely shutting this cock converts the whole into an engine for extinguishing fire, or for supplying distant places with water. It is scarcely necessary to add, that for these services it will be proper to connect an air-vessel with some convenient part of the rising-pipe, in order that the current of the water may be continual.

We have frequently spoken of the advantages of a continued current in the main pipe. In all great works a considerable degree of uniformity is produced by the manner of disposing the actions of the different pumps; for it is very rarely that a machine works but one pump. In order to maintain some uniformity in the resistance, that it may not all be opposed at once to the moving power, with intervals of total inaction, which would produce a very hobbling motion, it is usual to distribute the work into portions which succeed alternately; and thus both diminish the strain, and give greater uniformity of action, and frequently enable a natural power which we can command, to perform a piece of work, which would be impossible if the whole resistance were opposed at once. In all pump machines therefore we are obviously directed to construct them so that they may give motion to at least two pumps, which work alternately. By this means a much greater uniformity of current is produced in the main pipe. It will be rendered still more uniform if four are employed, succeeding each other at the interval of one quarter of the time of a complete stroke.

But ingenious men have attempted the same thing with a single pump, and many different constructions for this purpose have been proposed and executed. The thing is not of much importance, nor of great research. We shall content ourselves therefore with the description of one that appears to us the most perfect both in respect of simplicity and effect.

II. It consists of a working-barrel AB (Fig. 15.) close at both ends. The piston C is solid, and the rod OP passes through a collar of leathers in the plate, which closes the upper end of the working-barrel. This barrel communicates laterally with two pipes H, K; the communications *m* and *n* being as near to the top and bottom of the barrel as possible. Adjoining to the passage *m* are two valves F and G opening upwards. Similar valves accompany the passage *n*. The two pipes H and K unite in a larger rising pipe L. They are all represented as in the same plane; but the upper ends must be be sent backwards, to give room for the motion of the piston-rod OP.

Suppose the piston close to the entry of the lateral pipe *n*, and that it is drawn up, it compresses the air above it, and drives it through the valve G, where it escapes along the rising pipe; at the same time it rarefies the air in the space below it. Therefore the weight of the atmosphere shuts the valve E, and causes the water of the cistern to rise through the valve D, and fill the lower part of the pump. When the piston is pushed down again, this water is first driven through the valve E, because D immediately shuts; and then most of the air which was in this part of the pump at the beginning goes up through it, some of the water coming back in its stead. In the meantime, the air which remained in the upper part of the pump after the ascent of the piston is rarefied by its descent; because the valve G shuts as soon as the piston begins to descend, the valve F opens, the air in this suction-pipe F*f* expands into the barrel, and the water rises into the pipes by the pressure of the atmosphere. The next rise of the piston must bring more water into the lower part of the barrel, and must drive a little more air through the valve G, namely, part of that which had come out of the suction-pipe F*f*; and the next descent of the piston must drive more water into the rising pipe H, and along with it most if not all of the air which remained below the piston, and must rarefy still more the air remaining above the piston; and more water will come in through the pipe F*f*, and get into the barrel. It is evident that a few repetitions will at last fill the barrel on both sides of the piston with water. When this is accomplished, there is no difficulty in perceiving how, at every rise of the piston, the water of the cistern will come in by the valve D, and the water in the upper part of the barrel will be driven through the valve G; and, in every descent of the piston, the water of the cistern will come into the barrel by the valve F, and the water below the piston will be driven through the valve E; and thus there will be a continual influx into the barrel through the valves D and F, and a continual discharge along the rising pipe L through the valves E and G.

This machine is, to be sure, equivalent to two forcing-pumps, although it has but one barrel and one piston; but it has no sort of superiority. It is not even more economical in most cases; because we apprehend that the additional workmanship will fully compensate for the barrel and piston that is saved. There is indeed a saving in the rest of the machinery, because one lever produces both motions. We cannot therefore say that it is inferior to two pumps; and we acknowledge that there is some ingenuity in the contrivance.

We recommend to our readers the perusal of Belidor's *Architecture Hydraulique*, where is to be found a great variety of combinations and forms of the simple pumps; but we must caution them with respect to his theories, which in this article are extremely defective. Also in Leupold's *Theatrum Machinarum Hydraulicarum*, there is a prodigious variety of all kinds of pumps, many of them very singular and ingenious, and many which have particular advantages, which may suit local circumstances, and give them a preference. But it would be improper to swell an article of this kind with so many peculiarities; and a person who makes himself master of the principles delivered here in sufficient detail, can be at no loss to suit a pump to his particular views, or to judge of the merit of such as may be proposed to him.

We must now take notice of some very considerable and important varieties in the form and contrivance of the essential parts of a pump.

III. The forcing-pump is sometimes of a very different form from that already described. Instead of a piston, which applies itself to the inside of the barrel, and slides up and down in it, there is a long cylinder POQ (Fig. 16.) nicely turned and polished on the outside, and of a diameter somewhat less than the inside of the barrel. This cylinder (called a PLUNGER) slides through a collar of leathers on the top of the working-barrel, and is constructed as follows: The top of the barrel terminates in a flanch *a b*, pierced with four holes for receiving screw-bolts. There are two rings of metal, *c d*, *e f*, of the same diameter, and having holes corresponding to those in the flanch. Four rings of soft leather, of the same size, and similarly pierced with holes, are well soaked in a mixture of oil, tallow, and a little rosin. Two of these leather rings are laid on the pump flanch, and one of the metal rings above them. The plunger is then thrust down through them, by which it turns their inner edges downwards. The other two rings are then slipped on at the top of the plunger, and the second metal ring is put over them, and then the whole are slid down to the metal ring. By this the inner edges of the last leather rings are turned upwards. The three metal rings are now forced together by the screwed bolts; and thus the leathern rings are strongly compressed between them, and made to grasp the plunger so closely that no pressure can force the water through between. The upper metal ring just allows the plunger to pass through it, but without any play; so that the turned-up edges of the leathern rings do not come up between the plunger and the upper metal ring, but are lodged in a little conical taper, which is given to the inner edge of the upper plate, its hole being wider below than above. It is on this trifling circumstance that the great tightness of the collar depends. To prevent the leathers from shrinking by drought, there is usually a little cistern formed round the head of the pump, and kept full of water. The plunger is either forced down by a rod from a working-beam, or by a set of metal weights laid on it, as is represented in the figure.

It is hardly necessary to be particular in explaining the operation of this pump. When the plunger is at the bottom of the barrel, touching the fixed valve M with its lower extremity, it almost completely fills it. That it may do it completely, there is sometimes a small pipe RSZ branching out from the top of the barrel, and fitted with a cock at S. Water is admitted till the barrel is completely filled, and the cock is then shut. Now when the plunger is drawn up, the valve N in the rising pipe must remain shut by the pressure of the atmosphere, and a void must be made in the barrel. Therefore the valve M on the top of the suction-pipe must be opened by the elasticity of the air in this pipe, and the air must expand into the barrel; and, being no longer a balance for the atmosphere, the water in the cistern must be forced into the suction-pipe, and rise in it to a certain height. When the plunger descends, it must drive the water through the valve N (for the valve M will immediately shut), and along with it most of the air which had come into the barrel. And as this air occupied the upper part of the barrel, part of it will remain when the plunger has reached the bottom; but a stroke or two will expel it all, and then every succeeding stroke of the descending piston will drive the water along the rising pipe, and every ascent of the plunger will be followed by the water from the cistern.

The advantage proposed by this form of piston is, that it may be more accurately made and polished than the inside of a working-barrel, and it is of much easier repair.

Yet we do not find that it is much used, although an invention of last century (we think by Sir Samuel Morland), and much praised by the writers on these subjects.

It is easy to see that the sucking-pump may be varied in the same way. Suppose this plunger to be open both at top and bottom, but the bottom filled with a valve opening upward. When this is pushed to the bottom of the barrel, the air which it tends to compress lifts the valve (the lateral pipe FIK being taken away and the passage shut up), and escapes through the plunger. When it is drawn up, it makes the same rarefaction as the solid plunger, because the valve at O shuts, and the water will come up from the cistern as in the former case. If the plunger be now thrust down again, the valve M shuts, the valve O is forced open, and the plunger is filled with water. This will be lifted by it during its next ascent; and when it is pushed down again, the water which filled it must now be pushed out, and will flow over its sides into the cistern at the head of the barrel. Instead of making the valve at the bottom of the piston, it may be made at the top; but this disposition is much inferior, because it cannot rarefy the air in the barrel one half. This is evident; for the capacity of the barrel and plunger together cannot be twice the capacity of the barrel.

IV. It may be made after a still different form, as represented in Fig. 17. Here the suction-pipe CO comes up through a cistern KMNL deeper or longer than the intended stroke of the piston, and has a valve C at top. The piston, or what acts in lieu of it, is a tube AHGB, open at both ends, and of a diameter somewhat larger than that of the suction-pipe. The interval between them is filled up at HG by a ring or belt of soft leather, which is fastened to the *outer* tube, and moves up and down with it, sliding along the smoothly polished surface of the suction-pipe with very little friction. There is a valve I on the top of this piston, opening upwards. Water is poured into the outer cistern.

The outer cylinder or piston being drawn up from the bottom, there is a great rarefaction of the air which was between them, and the atmosphere presses the water up through the suction-pipe to a certain height; for the valve I keeps shut by the pressure of the atmosphere and its own weight. Pushing down the piston causes the air, which had expanded from the suction-pipe into the piston, to escape through the valve I; drawing it up a second time, allows the atmosphere to press more water into the suction-pipe, to fill it, and also part of the piston. When this is pushed down again, the water which had come through the valve C is now forced out through the valve I into the cistern KMNL, and now the whole is full of water. When, therefore, the piston is drawn up, the water follows, and fills it, if not 33 feet above the water in the cistern; and when it is pushed down again, the water which filled the piston is all thrown out into the cistern; and after this it delivers its full contents of water every stroke. The water in the cistern KMNL effectually prevents the entry of any air between the two pipes; so that a very moderate compression of the belt of soft leather at the mouth of the piston cylinder is sufficient to make all perfectly tight.

It might be made differently. The ring of leather might be fastened round the top of the inner cylinder at DE, and slide on the inside of the piston cylinder; but the first form is most easily executed. Muschenbroeck has given a figure of this pump in his large system of natural philosophy, and speaks very highly of its performance. But we do not see any advantage which it possesses over the common sucking-pump. He indeed says that it is without friction, and makes no mention of the ring of leather between the two cylinders. Such a pump will raise water extremely well to a small height, and it seems to have been a model only which he had examined: but if the suction-pipe is long, it will by no means do without the leather; for, on drawing up the piston, the water of the upper cistern will rise between the pipes, and fill the piston, and none will come up through the suction-pipe.

We may take this opportunity of observing, that the many ingenious contrivances of pumps without friction are of little importance in great works; because the friction which is completely sufficient to prevent all escape of water in a well-constructed pump is but a very trifling part of the whole force. In the great pumps which are used in mines, and are worked by a steam-engine, it is very usual to make the pistons and valves without any leather whatever. The working-barrel is bored truly cylindrical, and the piston is made of metal of a size that will just pass along it without sticking. When this is drawn up with the velocity competent to a properly loaded machine, the quantity of water which escapes round the piston is insignificant. The piston is made without leathers, not to avoid friction, which is also insignificant in such works; but to avoid the necessity of frequently drawing it up for repairs through such a length of pipes.

V. If a pump absolutely without friction is wanted, the following seems preferable for simplicity and performance to any we have seen, when made use of in proper situations. Let NO (Fig. 18.) be the surface of the water in the pit, and K the place of delivery. The pit must be as deep in water as from K to NO. ABCD is a wooden trunk, round or square, open at both ends, and having a valve P at the bottom. The top of this trunk must be on a level with K, and has a small cistern EADF. It also communicates laterally with a rising pipe GHK, furnished with a valve at H opening upwards. LM is a beam of timber so fitted to the trunk as to fill it without sticking, and is of at least equal length. It hangs by a chain from a working-beam, and is loaded on the top with weights exceeding that of the column of water which it displaces. Now suppose this beam allowed to descend from the position in which it is drawn in the figure, the water must rise all around it, in the crevice which is between it and the trunk, and also in the rising pipe; because the valve P shuts, and H opens; so that when the plunger has got to the bottom, the water will stand at the level of K. When the plunger is again drawn up to the top by the action of the moving power, the water sinks again in the trunk, but not in the rising pipe, because it is stopped by the valve H. Then allowing the plunger to descend again, the water must again rise in the trunk to the level of K, and it must now flow out at K; and the quantity discharged will be equal to the part of the beam below the surface of the pit-water, deducting the quantity which fills the small space between the beam and the trunk. This quantity may be reduced almost to nothing; for if the inside of the trunk and the outside of the beam be made tapering, the beam may be let down till they exactly fit; and as this may be done in square work, a good workman can make it exceedingly accurate. But in this case, the lower half of the beam and trunk must not taper; and this part of the trunk must be of sufficient width round the beam to allow free passage into the rising pipe. Or, which is better, the rising pipe must branch off from the bottom of the trunk. A discharge may be made from the cistern EADF, so that as little water as possible may descend along the trunk when the piston is raised.

One great excellence of this pump is, that it is perfectly free from all the deficiencies which in common pumps result from want of being air-tight. Another is, that the quantity of the water raised is precisely equal to the power expended; for any want of accuracy in the work, while it occasions a diminution of the quantity of water discharged, makes an equal diminution in the weight which is necessary for pushing down the plunger. We have seen a machine consisting of two such pumps suspended from the arms of a long beam, the upper side of which was formed into a walk with a rail on each side. A man stood on one end till it got to the bottom, and then walked soberly up to the other end, the inclination being about twenty-five degrees at first, but gradually diminished as he went along, and changed the load of the beam. By this means he made the other end go to the bottom, and so on alternately, with the easiest of all exertions, and what we are most fitted for by our structure. With this machine, a very feeble old man, weighing 110 pounds, raised 7 cubic feet of water 11½ feet high in a minute, and continued working 8 or 10 hours every day. A stout young man, weighing nearly 135 pounds, raised 8⅓ to the same height; and when he carried 30 pounds conveniently slung about him, he raised 9¼ feet to this height, working 10 hours a-day without fatiguing himself. This exceeds Desagulier's maximum of a hogshead of water 10 feet high in a minute, in the proportion of 9 to 7 nearly. It is limited to very moderate heights; but in such situations it is very effectual. It was the contrivance of an untaught labouring man, possessed of uncommon mechanical genius.

VI. The most ingenious contrivance of a pump without friction is that of Mr Haskins, described by Desaguliers, and called by him the *Quicksilver Pump*. Its construction and mode of operation are pretty complicated; but the following preliminary observations will, we hope, render it abundantly plain.

Let *i l m k* (Fig. 19.) be a cylindrical iron pipe, about six feet long, open at top. Let *e g h f* be another cylinder, connected with it at the bottom, and of smaller diameter. It may either be solid, or, if hollow, it must be close at top. Let *a c d b* be a third iron cylinder, of an intermediate diameter, so that it may move up and down between the other two without touching either, but with as little interval as possible. Let this middle cylinder communicate by means of the pipe AB, with the upright pipe FE, having valves C and D (both opening upwards) adjoining to the pipe of communication. Suppose the outer cylinder suspended by chains from the end of a working-beam, and let mercury be poured into the interval between the three cylinders till it fills the space to *o p*, about ¾ of their height. Also suppose that the lower end of the pipe FE is immersed into a cistern of water, and that the valve D is less than 33 feet above the surface of this water.

Now suppose a perforation made somewhere in the pipe AB, and a communication made with an air-pump. When the air-pump is worked, the air contained in CE, in AB, and in the space between the inner and middle cylinders, is rarefied, and is abstracted by the air-pump; for the valve D immediately shuts. The pressure of the atmosphere will cause the water to rise in the pipe CE, and will cause the mercury to rise between the inner and middle cylinders, and sink between the outer and middle cylinders. Let us suppose mercury 12 times heavier than water: then for every foot that the water rises in EC, the level between the outside and inside mercury will vary an inch; and if we suppose DE to be 30 feet, then if we can rarefy the air so as to raise the water to D, the outside mercury will be depressed to *q*, *r*, and the inside mercury will have risen to *s*, *t*, *s q* and *t r*, being about 30 inches. In this state of things, the water will run over by the pipe BA, and every thing will remain nearly in this position. The columns of water and mercury balance each other, and balance the pressure of the atmosphere.

While things are in this state of equilibrium, if we allow the cylinders to descend a little, the water will rise in the pipe FE, which we may now consider as a suction-pipe; for by this motion the capacity of the whole is enlarged, and therefore the pressure of the atmosphere will still keep it full, and the situation of the mercury will again be such that all shall be in equilibrio. It will be a little lower in the inside space and higher in the outside.

Taking this view of things, we see clearly how the water is supported by the atmosphere at a very considerable height. The apparatus is analogous to a syphon which has one leg filled with water and the other with mercury. But it was not necessary to employ an air-pump to fill it. Suppose it again empty, and all the valves shut by their own weight. Let the cylinders descend a little. The capacity of the spaces below the valve D is enlarged, and therefore the included air is rarefied, and some of the air in the pipe CE must diffuse itself into the space quitted by the inner cylinder. Therefore the atmosphere will press some water up the pipe FE, and some mercury into the inner space between the cylinders. When the cylinders are raised again, the air which came from the pipe CE would return into it again, but is prevented by the valve C.—Raising the cylinders to their former height would compress this air; it therefore lifts the valve D, and escapes. Another depression of the cylinders will have a similar effect. The water will rise higher in FC, and the mercury in the inner space; and then, after repeated strokes, the water will pass the valve C, and fill the whole apparatus, as the air-pump had caused it to do before. The position of the cylinders, when things are in this situation, is represented in Fig. 20, the outer and inner cylinders in their lowest position having descended about 30 inches. The mercury in the outer space stands at *q*, *r*, a little above the middle of the cylinders, and the mercury in the inner space is near the top *t s* of the inner cylinder. Now let the cylinders be drawn up. The water above the mercury cannot get back again through the valve C, which shuts by its own weight. We therefore attempt to compress it; but the mercury yields, and descends in the inner space, and rises in the outer, till both are quickly on a level, about the height *v v*. If we continue to raise the cylinders, the compression forces out more mercury, and it now stands lower in the inner than in the outer space. But that there may be something to balance this inequality of the mercurial columns, the water goes through the valve D, and the equilibrium is restored when the height of the water in the pipe ED above the surface of the internal mercury is 12 times the difference of the mercurial columns (on the former supposition of specific gravity). If the quantity of water is such as to rise two feet in the pipe ED, the mercury in the outer space will be two inches higher than that in the inner space. Another depression of the cylinders will again enlarge the space within the apparatus, the mercury will take the position of Fig. 19. and more water will come in. Raising the cylinders will send this water four feet up the pipe ED, and the mercury will be four inches higher in the inner than in the outer space. Repeating this operation, the water will be raised still higher in DE; and this will go on till the mercury in the outer space reaches the top of the cylinder; and this is the limit of the performance. The dimensions with which we set out will enable the machine to raise the water about 30 feet in the pipe ED; which, added to the 30 feet of CF, makes the whole height above the pit-water 60 feet. By making the cylinders longer, we increase the height of FD. This machine must be worked with great attention, and but *slowly*; for at the beginning of the forcing stroke the mercury very rapidly sinks in the inner space and rises in the outer, and will dash out and be lost. To prevent this as much as possible, the outer cylinder terminates in a sort of cup or dish, and the inner cylinder should be tapered atop.

The machine is exceedingly ingenious and refined; and there is no doubt of its performance exceeding that of any other pump which raises the water to the same height, because friction is completely avoided, and there can be no want of tightness of the piston. But this is all its advantage; and, from what has been observed, it is but trifling. The expense would be enormous; for with whatever care the cylinders are made, the interval between the inner and outer cylinders must contain a very great quantity of mercury. The middle cylinder must be made of iron plate, and must be without a seam, for the mercury would dissolve every solder. For such reasons, it has never come into general use. But it would have been unpardonable to have omitted the description of an invention which is so original and ingenious; and there are some occasions where it may be of great use, as in nice experiments for illustrating the theory of hydraulics, it would give the finest pistons for measuring the pressures of water in pipes, &c. It is on precisely the same principle that the cylinder bellows, described in the article PNEUMATICS, are constructed.

We beg leave to conclude this part of the subject with the description of a pump without friction, which may be constructed in a variety of ways by any common carpenter, without the assistance of the pump-maker or plumber, and will be very effective for raising a great quantity of water to small heights, as in draining marshes, marle pits, quarries, &c. or even for the service of a house.

VII. ABCD (Fig. 21.) is a square trunk of carpenter's work open at both ends, and having a little cistern and spout at top. Near the bottom there is a partition made of board, perforated with a hole E, and covered with a clack. *ffff* represents a long cylindrical bag or pudding, made of leather or of double canvass, with a fold of thin leather such as sheep-skin between the canvass bags. This is firmly nailed to the board E with soft leather between. The upper end of this bag is fixed on a round board, having a hole and valve F. This board may be turned in the lathe with a groove round its edge, and the bag fastened to it by a cord bound tight round it. The fork of the piston-rod FG is firmly fixed into this board; the bag is kept distended by a number of wooden hoops or rings of strong wire *ff*, *ff*, *ff*, &c. put into it at a few inches distance from each other. It will be proper to connect these hoops before putting them in, by three or four cords from top to bottom, which will keep them at their proper distances. Thus will the bag have the form of a barber's bellows powder-puff. The distance between the hoops should be about twice the breadth of the rim of the wooden ring to which the upper valve and piston-rod are fixed.

Now let this trunk be immersed in the water. It is evident that if the bag be stretched from the compressed form which its own weight will give it by drawing up the piston-rod, its capacity will be enlarged, the valve F will be shut by its own weight, the air in the bag will be rarefied, and the atmosphere will press the water into the bag. When the rod is thrust down again, this water will come out by the valve F, and fill part of the trunk. A repetition of the operation will have a similar effect; the trunk will be filled, and the water will at last be discharged by the spout.

Here is a pump without friction, and perfectly tight. For the leather between the folds of canvass renders the bag impervious both to air and water. And the canvass has very considerable strength. We know from experience that a bag of six inches diameter, made of sail-cloth No 3, with a sheep-skin between, will bear a column of 15 feet of water, and stand six hours work *per* day for a month without failure, and that the pump is considerably superior in effect to a common pump of the same dimensions. We must only observe, that the length of the bag must be three times the intended length of the stroke; so that when the piston-rod is in its highest position, the angles or ridges of the bag may be pretty acute. If the bag be more stretched than this, the force which must be exerted by the labourer becomes much greater than the weight of the column of water which he is raising. If the pump be laid aslope, which is very usual in these occasional and hasty drawings, it is necessary to make a guide for the piston-rod within the trunk, that the bag may play up and down without rubbing on the sides, which would quickly wear it out.

The experienced reader will see that this pump is very like that of Gosset and De la Deuille, described by Belidor, vo II. p. 130, and most writers on hydraulics. It would be still more like it, if the bag were on the under side of the partition E, and a valve placed farther down the trunk. But we think that our form is greatly preferable in point of strength. When in the other situation, the column of water lifted by the piston tends to *burst* the bag, and this with a great force, as the intelligent reader well knows.—But in the form recommended here, the bag is *compressed*, and the strain on each part may be made much less than that which tends to burst a bag of six inches diameter. The nearer the rings are placed to each other the smaller will the strain be.

The same bag-piston may be employed for a forcing-pump, by placing it below the partition, and inverting the valve; and it will then be equally strong, because the resistance in this case too will act by compression.

We now come naturally to the consideration of the different forms which may be given to the pistons and valves of a pump. A good deal of what we have been describing already is reducible to this head; but, having a more general appearance, changing as it were the whole form and structure of the pump, it was not improper to keep these things together.

The great desideratum in a piston is, that it be as tight as possible, and have as little friction as is consistent with this indispensable quality. We have already said, that the common form, when carefully executed, has these properties in an eminent degree. And accordingly this form has kept its ground amidst all the improvements which ingenious artists have made. Mr Belidor, an author of the first reputation, has given the description of a piston which he highly extols, and is undoubtedly a very good one, constructed from principle, and extremely well composed.

It consists of a hollow cylinder of metal *g h* (Fig. 22.) pierced with a number of holes, and having at top a flanch AB, whose diameter is nearly equal to that of the working-barrel of the pump. This flanch has a groove round it. There is another flanch IK below, by which this hollow cylinder is fastened with bolts to the lower end of the piston, represented in Fig. 23. This consists of a plate CD, with a grooved edge similar to AB, and an intermediate plate which forms the seat of the valve. The composition of this part is better understood by inspecting the figure than by any description. The piston-rod HL is fixed to the upper plate by bolts through its different branches at G, G. This metal body is then covered with a cylindrical bag of leather, fastened on it by cords bound round it, filling up the grooves in the upper and lower plates. The operation of the piston is as follows:

A little water is poured into the pump, which gets past the sides of the piston, and lodges below in the fixed valve. The piston being pushed down dips into this water, and it gets into it by the valve. But as the piston in descending compresses the air below it, this compressed air also gets into the inside of the piston, swells out the bag which surrounds it, and compresses it to the sides of the working-barrel. When the piston is drawn up again, it must remain tight, because the valve will shut and keep in the air in its most compressed state; therefore the piston must perform well during the suction. It must act equally well when pushed down again, and acting as a forcer; for however great the resistance may be, it will affect the air within the piston to the same degree, and keep the leather close applied to the barrel. There can be no doubt therefore of the piston's performing both its offices completely; but we imagine that the adhesion to the barrel will be greater than is necessary: it will extend over the whole surface of the piston, and be equally great in every part of its surface; and we suspect that the friction will therefore be very great. We have very high authority for supposing that the adhesion of a piston of the common form, carefully made, will be such as will make it perfectly tight; and it is evident that the adhesion of Belidor's piston will be much greater, and it will be productive of worse consequences. If the leather bag is worn through in any one place, the air escapes, and the piston ceases to be compressed altogether; whereas in the common piston there will very little harm result from the leather being worn through in one place, especially if it project a good way beyond the base of the cone. We still think the common piston preferable. Belidor's piston would do much better inverted as the piston of a sucking-pump; and in this situation it would be equal, but not superior, to the common.

Belidor describes another forcing-piston, which he had executed with success, and prefers to the common wooden forcer. It consists of a metal cylinder or cone, having a broad flanch united to it at one end, and a similar flanch which is screwed on the other end. Between these two plates are a number of rings of leather strongly compressed by the two flanches, and then turned in a lathe like a block of wood, till the whole fits tight, when dry, into the barrel. It will swell, says he, and soften with the water, and withstand the greatest pressures. We cannot help thinking this but an indifferent piston. When it wears, there is nothing to squeeze it to the barrel. It may indeed be taken out and another ring or two of leather put in, or the flanches may be more strongly screwed together: but all this may be done with any kind of piston; and this has therefore no peculiar merit.

The following will, we presume, appear vastly preferable. ABCD (Fig. 24.) is the solid wooden or metal block of the piston; EF is a metal plate, which is turned hollow or dish-like below, so as to receive within it the solid block. The piston-rod goes through the whole, and has a shoulder above the plate EF, and a nut H below. Four screw-bolts, such as *i k*, *l m*, also go through the whole, having their heads *k*, *m* sunk into the block, and nuts above at *i*, *l*. The packing or stuffing, as it is termed by the workmen, is represented at NO. This is made as solid as possible, and generally consists of soft hempen twine well soaked in a mixture of oil, tallow, and rosin. The plate EF is gently screwed down, and the whole is then put into the barrel, fitting it as tight as may be thought proper. When it wears loose, it may be tightened at any time by screwing down the nuts *i l*, which cause the edges of the dish to squeeze out the packing, and compress it against the barrel to any degree.

The greatest difficulty in the construction of a piston is to give a sufficient passage through it for the water, and yet allow a firm support for the valve, and fixture for the piston-rod. We shall see presently that it occasions a considerable expense of the moving power to force a piston with a narrow perforation through the water lodged in the working-barrel. When we are raising water to a small height, such as 10 or 20 feet, the power so expended amounts to a fourth part of the whole, if the water-way in the piston is less than one-half of the section of the barrel, and the velocity of the piston two feet *per* second, which is very moderate. There can be no doubt, therefore, that metal pistons are preferable, because their greater strength allows much wider apertures.

The following piston, described and recommended by Belidor, seems as perfect in these respects as the nature of things will allow. We shall therefore describe it in the author's own words as a model, which may be adopted with confidence in the greatest works:

"The body of the piston is a truncated metal cone (Fig. 25.), having a small fillet at the greater end.

Fig. 26. shows the profile, and Fig. 27. the plan of its upper base; where appears a cross bar DD, pierced with an oblong mortise E for receiving the tail of the piston-rod. A band of thick and uniform leather AA (Fig. 26. and 28.) is put round this cone, and secured by a brass hoop BB firmly driven on its smaller end, where it is previously made thinner to give room for the hoop.

"This piston is covered with a leather valve, fortified with metal plates GG (Fig. 29.) These plates are wider than the hole of the piston, so as to rest on its rim. There are similar plates below the leather of a smaller size, that they may go into the hollow of the piston; and the leather is firmly held between the metal plates by screws H, H, which go through all. This is represented by the dotted circle IK. Thus the pressure of the incumbent column of water is supported by the plates GG, whose circular edges rest on the brim of the water-way, and thus straight edges rest on the cross bar DD of Fig. 26. and 27. This valve is laid on the top of the conical box in such a manner that its middle FF rests on the cross bar. To bind all together, the end of the piston-rod is formed like a cross, and the arms MN (Fig. 30.) are made to rest on the diameter FF of the valve, the tail EP going through the hole E in the middle of the leather, and through the mortise E of the cross bar of the box; and also through another bar QR (Fig. 28. and 29.) which is notched into the lower brim of the box. A key V is then driven into the hole T in the piston-rod; and this wedges all fast. The bar QR is made strong; and its extremities project a little, so as to support the brass hoop BB, which binds the leather band to the piston-box. The adjoining scale gives the dimensions of all the parts, as they were executed for a steam-engine near Condé, where the piston gave complete satisfaction."

This piston has every advantage of strength, tightness, and large water-way. The form of the valve (which has given it the name of the *butterfly-valve*) is extremely favourable to the passage of the water; and as it has but half the motion of a complete circular valve, less water goes back while it is shutting.

The following piston is also ingenious, and has a good deal of merit. OPPO (Fig. 31.) is the box of the piston, having a perforation Q, covered above with a flat valve K, which rests on a metal plate that forms the top of the box. ABCBA is a stirrup of iron to which the box is fixed by screws *a*, *a*, *a*, *a*, whose heads are sunk in the wood. This stirrup is perforated at C, to receive the end of the piston-rod, and a nut H is screwed on below to keep it fast. DEFED is another stirrup, whose lower part at DD forms a hoop like the sole of a stirrup, which embraces a small part of the top of the wooden box. The lower end of the piston-rod is screwed; and before it is put into the holes of the two stirrups (through which holes it slides freely) a broad nut G is screwed on it. It is then put into the holes, and the nut H firmly screwed up. The packing RR is then wound about the piston as tight as possible till it completely fills the working-barrel of the pump. When long use has rendered it in any degree loose, it may be tightened again by screwing down the nut G. This causes the ring DD to compress the packing between it and the projecting shoulder of the box at PP; and thus causes it to swell out, and apply itself closely to the barrel.

We shall add only another form of a perforated piston; which being on a principle different from all the preceding, will suggest many others; each of which will have its peculiar advantages. OO in Fig. 32. represents the box of this piston, fitted to the working-barrel in any of the preceding ways as may be thought best. AB is a cross-bar of four arms, which is fixed to the top of the box. CF is the piston-rod going through a hole in the middle of AB, and reaching a little way beyond the bottom of the box. It has a shoulder D, which prevents its going too far through. On the lower end there is a thick metal plate, turned conical on its upper side, so as to fit a conical seat PP in the bottom of the piston-box.

When the piston-rod is pushed down, the friction on the barrel prevents the box from immediately yielding. The rod therefore slips through the hole of the cross-bar AB. The plate E, therefore, detaches itself from the box. When the shoulder D presses on the bar AB, the box must yield, and be pushed down the barrels, and the water gets up through the perforation. When the piston-rod is drawn up again, the box does not move till the plate E lodged in the seat PP, and thus shuts the water-way; and then the piston lifts the water which is above it, and acts as the piston of a sucking-pump.

This is a very simple and effective construction, and makes a very tight valve. It has been much recommended by engineers of the first reputation, and is frequently used; and from its simplicity, and the great solidity of which it is capable, it seems very fit for great works. But it is evident that the water-way is limited to less than one-half of the area of the working-barrel. For if the perforation of the piston be one-half of the area, the diameter of the plate or ball EF must be greater; and therefore less than half the area will be left for the passage of the water by its sides.

We now come to consider the forms which may be given to the valves of a hydraulic engine.

The requisites of a valve are, that it shall be tight, of sufficient strength to resist the great pressures to which it is exposed, that it afford a sufficient passage for the water, and that it do not allow much to go back while it is shutting.

We have not much to add to what has been said already on this subject. The valves which accompany the pump of Fig. 5. are called *clack-valves*, and are all the most obvious and common; and the construction described on that occasion is as perfect as any. We only add, that as the leather is at last destroyed at the hinge by such incessant motion, and it is troublesome, especially in deep mines, and under water, to undo the joint of the pump in order to put in a new valve, it is frequently annexed to a box like that of a piston, made a little conical on the outside, so as to fit a conical seat made for it in the pipe, as represented in Fig. 26. and it has an iron handle like that of a bucket, by which it can be laid hold of by means of a long grapple or plug-hook let down from above. Thus it is drawn up; and being very gently tapered on the sides, it sticks very fast in its place.

The only defect of this valve is, that by opening very wide when pushed up by the stream of water, it allows a good deal to go back during its shutting again. In some great machines, which are worked by a slow-turning crank, the return of the piston is so very slow, that a sensible loss is incurred by this; but it is nothing like what Dr Desaguliers says, one-half of a cylinder whose height is equal to the diameter of the valve.—For in such machines, the last part of the upward stroke is equally slow, and the velocity of the water through the valve exceedingly small, so that the valve is at this time almost shut.

The butterfly valve represented in Figs. 29, &c. is free from most of those inconveniences, and seems the most perfect of the clack-valves. Some engineers make their great valves of a pyramidal form, consisting of four clacks, whose hinges are in the circumference of the water-way, and which meets with their points in the middle, and are supported by four ribs which rise up from the sides, and unite in the middle. This is an excellent form, affording the most spacious water-way, and shutting very readily. It seems to be the best possible for a piston. The rod of the piston is branched out on four sides, and the branches go through the piston-box, and are fastened below with screws. These branches form the support of the four clacks. We have seen a valve of this form in a pump of six feet diameter, which discharged 20 hogsheads of water every stroke, and made 12 strokes in a minute, raising the water above 22 feet.

There is another form of valve, called the *button* or *tail-valve*. It consists of a plate of metal AB (Fig. 34.) turned conical, so as exactly to fit the conical cavity *a b* of its box. A tail CD projects from the under side, which passes through a cross-bar EF in the bottom of the box, and has a little knob at the end, to hinder the valve from rising too high.

This valve, when nicely made, is unexceptionable. It has great strength, and is therefore proper for all severe strains, and it may be made perfectly tight by grinding. Accordingly it is used in all cases where this is of indispensable consequence. It is most durable, and the only kind that will do for passages where steam or hot water is to go through. Its only imperfection is a small water-way; which, from what has been said, cannot exceed, nor indeed equal, one-half of the area of the pipe.

If we endeavour to enlarge the water-way, by giving the cone very little taper, the valve frequently sticks so fast in the seat that no force can detach them.—And this sometimes happens during the working of the machine; and the jolts and blows given to the machine in taking it to pieces, in order to discover what has been the reason that it has discharged no water, frequently detaches the valve, and we find it quite loose, and cannot tell what has deranged the pump. When this is guarded against, and the diminution of the water-way is not of very great consequence, this is the best form of a valve.

Analogous to this is the simplest of all valves, represented in Fig. 35. It is nothing more than a sphere of metal A, to which is fitted a seat with a small portion BC of a spherical cavity. Nothing can be more effectual than this valve; it always falls into its proper place, and in every position fits it exactly. Its only imperfection is the great diminution of the water-way. If the diameter of the sphere does not considerably exceed that of the hole, the touching parts have very little taper, and it is very apt to stick fast. It opposes much less resistance to the passage of the water than the flat under-surface of the button-valve. *N. B.*—It would be an improvement of that valve to give it a taper-shape below like a boy's top. The spherical valve must not be made too light, otherwise it will be hurried up by the water, and much may go back while it is returning to its place.

Belidor describes with great minuteness (vol. II. p. 221, &c.) a valve which unites every requisite. But it is of such nice and delicate construction, and its defects are so great when this exactness is not attained, or is impaired by use, that we think it hazardous to introduce it into a machine in a situation where an intelligent and accurate artist is not at hand. For this reason we have omitted the description, which cannot be given in few words, nor without many figures; and desire our curious readers to consult that author, or peruse Dr Desaguliers' translation of this passage. Its principle is precisely the same with the following rude contrivance, with which we shall conclude the descriptive part of this article:

Suppose ABCD (Fig. 36.) to be a square wooden trunk. EF is a piece of oak-board, exactly fitted to the trunk in an oblique position, and supported by an iron pin which goes through it at I, one-third of its length from the lower extremity E. The two ends of this board are bevelled, so as to apply exactly to the sides of the trunk. It is evident, that if a stream of water comes in the direction BA, its pressure on the part IF of this board will be greater than that upon EI. It will therefore force it up and rush through, making it stand almost parallel to the sides of the trunk. To prevent its rising so far, a pin must be put in its way. When this current of water changes its direction, the pressure on the upper side of the board being again greatest on the portion IF, it is forced back again to its former situation; and its two extremities resting on the opposite sides of the trunk, the passage is completely stopped. This board therefore performs the office of a valve; and this valve is the most perfect that can be, because it offers the freest passage to the water, and it allows very little to get back while it is shutting; for the part IE brings up half as much water as IF allows to go down. It may be made extremely tight, by fixing two thin fillets H and G to the sides of the trunk, and covering those parts of the board with leather which applies to them; and in this state it perfectly resembles Belidor's fine valve.

And this construction of the valve suggests, by the way, a form of an occasional pump, which may be quickly set up by any common carpenter, and will be very effectual in small heights. Let *a b c d e* (Fig. 36.) be a square box made to slide along this wooden trunk without shake, having two of its sides projecting upwards, terminating like the gable-ends of a house. A piece of wood *e* is mortised into these two sides, and to this the piston-rod is fixed. This box being furnished with a valve similar to the one below, will perform the office of a piston. If this pump be immersed so deep in the water that the piston shall also be under water, we scruple not to say that its performance will be equal to any. The piston may be made abundantly tight by covering its outside neatly with soft leather. And as no pipe can be bored with greater accuracy than a very ordinary workman can make a square trunk, we presume that this pump will not be very deficient even for a considerable suction.

We now proceed to the last part of the subject, to consider the motion of water in pumps, in reference to the force which must be employed. What we have hitherto said with respect to the force which must be applied to a piston, related only to the sustaining the water at a certain height; but in actual service we must not only do this, but we must discharge it at the place of delivery in a certain quantity; and this must require a force superadded to what is necessary for its mere support at this height.

This is an extremely intricate and difficult subject, and very imperfectly understood even by professed engineers. The principles on which this knowledge must be founded are of a much more abstruse nature than the ordinary laws of hydrostatics; and all the genius of Newton was employed in laying the foundation of this part of physical science. It has been much cultivated in the course of this century by the first mathematicians of Europe. Daniel and *John* Bernoulli have written very elaborate treatises on the subject, under the very apposite name of HYDRODYNAMICS; in which, although they have added little or nothing to the fundamental propositions established in some sort by Newton, and acquiesced in by them, yet they have greatly contributed to our progress in it by the *methods* which they have pursued in making application of those fundamental propositions to the most important cases. It must be acknowledged, however, that both these propositions, and the extensions given them by these authors, are supported by a train of argument that is by no means unexceptionable; and that they proceed on assumptions or postulates which are but nearly true in any case, and in many are inadmissible: and it remains to this hour a wonder or puzzle how these propositions and their results correspond with the phenomena which we observe.

But fortunately this correspondence does obtain to a certain extent. And it seems to be this correspondence chiefly which has given these authors, with Newton at their head, the confidence which they place in their respective principles and methods: for there are considerable differences among them in those respects; and each seems convinced that the others are in a mistake. D'Alembert and La Grange have greatly corrected the theories of their predecessors, and have proceeded on postulates which come much nearer to the real state of the case. But their investigations involve us in such an inextricable maze of analytical investigation, that even when we are again conducted to the light of day by the clue which they have given us, we can make no use of what we there discovered.

But this theory, imperfect as it is, is of great service. It generalizes our observations and experiments, and enables us to compose a *practical doctrine* from a heap of facts which otherwise must have remained solitary and unconnected, and as cumbersome in their application as the characters of the Chinese writing.

The fundamental proposition of this practical hydrodynamics is, that water, or any fluid contained in an open vessel of indefinite magnitude, and impelled by its weight only, will flow through a small orifice with the velocity which a heavy body would acquire by falling from the horizontal surface of the fluid. Thus, if the orifice is 16 feet under the surface of the water, it will issue with the velocity of 32 feet in a second.

Its velocity corresponding to any other depth h of the orifice under the surface, will be had by this easy proportion: As the square root of 16 is to the square root of h; so is 32 feet to the velocity required: or, alternately, $\sqrt{16} : 32 = \sqrt{h} : v$, and $v = \frac{32\sqrt{h}}{\sqrt{16}} = \frac{32}{4}\sqrt{h}, = 8\sqrt{h}$: that is, multiply the square root of the height in feet by eight, and the product is the required velocity.

On the other hand, it frequently occurs, that we want to discover the depth under the surface which will produce a known velocity v. Therefore $\sqrt{h} = \frac{v}{8}$, and $h = \frac{v^2}{64}$: that is, divide the square of the velocity by 64, and the quotient is the depth wanted in feet.

This proposition is sufficient for all our purposes. For since water is nearly a perfect fluid, and propagates all impressions undiminished, we can, in place of any pressure of a piston or other cause, substitute a perpendicular column of water whose weight is equal to this pressure, and will therefore produce the same efflux. Thus, if the surface of a piston is half a square foot, and it be pressed down with the weight of 500 pounds, and we would wish to know with what velocity it would cause the water to flow through a small hole, we know that a column of water of this weight, and of half a foot base, would be 16 feet high. And this proposition teaches us, that a vessel of this depth will have a velocity of efflux equal to 32 feet in a second.

If therefore our pressing power be of such a kind that it can continue to press forward the piston with the force of 500 pounds, the water will flow with this velocity, whatever be the size of the hole. All that remains is, to determine what change of *actual pressure* on the piston results from the motion of the piston itself, and to change the velocity of efflux in the subduplicate ratio of the change of actual pressure.

But before we can apply this knowledge to the circumstances which take place in the motion of water in pumps, we must take notice of an important modification of the fundamental proposition, which is but very obscurely pointed out by any good theory, but is established on the most regular and unexceptionable observation.

If the efflux is made through a hole in a thin plate, and the velocity is computed as above, we shall discover the quantity of water which issues in a second by observing, that it is a prism or cylinder of the length indicated by the velocity, and having its transverse section equal to that of the orifice. Thus, in the example already given, supposing the hole to be a square inch, the solid contents of this prism, or the quantity of water issuing in a second, is 1 × 32 × 12 cubic inches, or 384 cubic inches. This we can easily measure by receiving it in a vessel of known dimensions. Taking this method, we uniformly find a deficiency of nearly 38 parts in 100; that is, if we should obtain 100 gallons in any number of seconds, we shall in fact get only 62. This is a most regular fact, whether the velocities are great or small, and whatever be the size and form of the orifice. The deficiency increases indeed in a very minute degree with the velocities. If, for instance, the depth of the orifice be one foot, the discharge is $\frac{6213}{10000}$; if it be 15 feet, the discharge is $\frac{6172}{10000}$.

This deficiency is not owing to a diminution of velocity; for the velocity may be easily and accurately measured by the distance to which the jet will go, if directed horizontally. This is found to correspond very nearly with the proposition, making a very small allowance for friction at the border of the hole, and for the resistance of the air. Sir Isaac Newton ascribed the deficiency with great justice to this, that the lateral columns of water, surrounding the column which is incumbent on the orifice, press towards the orifice, and contribute to the expense equally with that column. These lateral filaments, therefore, issue obliquely, crossing the motion of the central stream, and produce a contraction of the jet; and the whole stream does not acquire a parallel motion and its ultimate velocity till it has got to some distance from the orifice. Careful observation showed him that this was really the case. But even his genius could not enable him to ascertain the motion of the lateral filaments by theory, and he was obliged to measure every thing as he saw it. He found the diameter of the jet at the place of the greatest contraction to be precisely such as accounted for the deficiency. His explication has been unanimously acquiesced in; and experiments have been multiplied to ascertain all those circumstances which our theory cannot determine *à priori*. The most complete set of experiments are those of Michelotti, made at Turin at the expense of the Prince of Piedmont. Here jets were made of 1, 2, 3, and 4 inches diameter; and the water received into cisterns most accurately formed of brick, and lined with stucco. It is the result of these experiments which we have taken for a measure of the deficiency.

We may therefore consider the water as flowing through a hole of this contracted dimension, or substitute this for the real orifice in all calculations. For it is evident that if a mouth-piece (so to call it) were made, whose internal shape precisely tallied with the form which the jet assumes, and if this mouth-piece be applied to the orifice, the water will flow out without any obstruction. The vessel may therefore be considered as really having this mouth-piece.

Nay, from this we derive a very important observation, "that if, instead of allowing the water to flow through a hole of an inch area made in a thin plate, we make it flow through a hole in a thick plank, so formed that the external orifice shall have an inch area, but be widened internally agreeably to the shape which nature forms, both the velocity and quantity will be that which the fundamental proposition determines. Michelotti measured with great care the form of the great jets of three and *four* inches diameter, and found that the bounding curve was an elongated trochoid. He then made a mouth-piece of this form for his jet of one inch, and another for his jet of two inches; and he found the discharges to be $\frac{979}{1000}$ and $\frac{983}{1000}$; and he, with justice, ascribed the trifling deficiency which still remained, partly to friction and partly to his not having exactly suited his mouth-piece to the natural form. We imagine that this last circumstance was the sole cause: for, in the first place, the water in his experiments, before getting at his jet-holes, had to pass along a tube of eight inches diameter. Now a jet of four inches bears too great a proportion to this pipe; and its narrowness undoubtedly hindered the lateral columns from contributing to the efflux in their due proportion, and therefore rendered the jet less convergent. And, in the next place, there can be no doubt (and the observations of Daniel Bernoulli confirm it) but that this convergency begins within the vessel, and perhaps at a very considerable distance from the orifice. And we imagine, that if accurate observations could be made on the motion of the remote lateral particles within the vessel, and an internal mouth-piece were shaped according to the curve which is described by the remotest particle that we can observe, the efflux of water would almost perfectly tally with the theory. But indeed the coincidence is already sufficiently near for giving us very valuable information. We learn that the quantity of water which flows through a hole, in consequence of its own weight, or by the action of any force, may be increased one half by properly shaping the passage to this hole; for we see that it may be increased from 62 to near 99.

But there is another modification of the efflux, which we confess our total incapacity to explain. If the water issues through a hole made in a plate whose thickness is about twice the diameter of the hole, or, to express it better, if it issues through a pipe whose length is about twice its diameter, the quantity discharged is nearly $\frac{82}{100}$ of what results from the proposition. If the pipe be longer than this, the quantity is diminished by friction, which increases as the length of the pipe increases. If the pipe be shorter the water will not fill it, but detaches itself at the very entry of the pipe, and flows with a contracted jet. When the pipe is of this length, and the extremity is stopped with the finger, so that it begins to flow with a full mouth, no subsequent contraction is observed; but merely striking on the pipe with a key or the knuckle is generally sufficient to detach the water in an instant from the sides of the pipe, and reduce the efflux to $\frac{62}{100}$.

This effect is most unaccountable. It certainly arises from the mutual adhesion or attraction between the water and the sides of the pipe; but how this, acting at right angles to the motion, should produce an increase from 62 to 82, nearly $\frac{1}{3}$, we cannot explain. It shows, however, the prodigious force of this attraction, which in the space of two or three inches is able to communicate a great velocity to a very great body of water. Indeed the experiments on capillary tubes show that the mutual attraction of the parts of water is some thousands of times greater than their weight.

We have only further to add, that every increase of pipe beyond two diameters is accompanied with a diminution of the discharge; but in what ratio this is diminished it is very difficult to determine. We shall only observe at present that the diminution is very great. A pipe of 2 inches diameter and 30 feet long has its discharge only [illegible] of what it would be if only 4 inches long. If its length be 60 feet, its discharge will be no more than [illegible]. A pipe of 1 inch diameter would have a discharge of [illegible] and [illegible] in the same situation. Hence we may conclude that the discharge of a 4-inch pipe of 30 feet long will not exceed [illegible] of what it would be if only 8 inches long. This will suffice for our present purposes; and the determination of the velocities and discharges in long conduits from pump-machines will be found in our dissertation on *Water-Works*. At present we shall confine our attention to the pump itself, and to what will contribute to its improvement.

Before we can proceed to apply this fundamental proposition to our purpose, we must anticipate in a loose way a position of continual use in the construction of water-works.

Let water be supposed stagnant in a vessel EFGH (Fig. 37.), and let it be allowed to flow out by a cylindrical pipe HIKL, divided by any number of partitions B, C, D, &c. Whatever be the areas B, C, D, of these orifices, the velocity in the intermediate parts of the pipe will be the same; for as much passes through any one orifice in a second as passes through any other in the same time, or through any section of the intervening pipe. Let this velocity in the pipe be V, and let the area of the pipe be A. The velocity in the orifices B, C, D, must be $\frac{VA}{B}$,

$\frac{VA}{C}$, $\frac{VA}{D}$, &c. Let g be the velocity acquired in a second by a heavy body. Then, by the general proposition, the height of water in the vessel which will produce the velocity $\frac{VA}{B}$ in the first orifice alone, is $\frac{V^2A^2}{2gB^2}$. After this passage the velocity is again reduced to V in the middle of the space between the first and second orifices. In the second orifice this velocity is changed to $\frac{VA}{C}$. This alone would have required a height of water $\frac{V^2A^2}{2gC^2}$. But the water is already moving with the velocity V, which would have resulted from a height of water in vessel (which we shall, in the language of the art, call the *Head of Water*) equal to $\frac{V^2}{2g}$. Therefore there is only required a head of water $\frac{V^2A^2}{2gC^2}-\frac{V^2}{2g}$, or $\frac{V^2}{2g}\times\left(\frac{A^2}{C^2}-1.\right)$ Therefore the whole height necessary for producing the efflux through both orifices, so as still to preserve the velocity V in the intervening pipe, is $\frac{V^2}{2g}\times\overline{\frac{A^2}{B^2}+\frac{A^2}{C^2}-1}$. In like manner the third orifice D would alone require a head of water $\frac{V^2}{2g}\times\overline{\frac{A^2}{D^2}-1}$; and all the three would require a head $\frac{V^2}{2g}\times\overline{\frac{A^2}{B^2}+\frac{A^2}{C}+\frac{A^2}{D^2}-2}$. By this induction may easily be seen what head is necessary for producing the efflux through any number of orifices.

Let the expense or quantity of water discharged in an unit of time (suppose a second) be expressed by the symbol Q. This is measured by the product of the velocity by the area of the orifice, and is therefore $= \mathrm{VA}$, or $\frac{\mathrm{VA}}{\mathrm{B}} \times \mathrm{B}$, or $\frac{\mathrm{VA}}{\mathrm{C}} \times \mathrm{C}$, &c. and $\mathrm{V}^2 = \frac{\mathrm{Q}^2}{\mathrm{A}^2}$. Therefore we may compute the head of water (which we shall express by H) in reference to the quantity of water discharged, because this is generally the interesting circumstance. In this view we have $\mathrm{H} = \frac{\mathrm{Q}^2}{2g\,\mathrm{A}} \times \overline{\frac{\mathrm{A}^2}{\mathrm{B}^2} + \frac{\mathrm{A}^2}{\mathrm{C}^2} + \frac{\mathrm{A}^2}{\mathrm{D}^2} - 2}$: which shows that the head of water necessary for producing the discharge increases in the proportion of the square of the quantity of water which is discharged.

These things being premised, it is an easy matter to determine the motion of water in a pump, and the quantity discharged, resulting from the action of any force on the piston, or the force which must be applied to the piston in order to produce any required motion or quantity discharged. We have only to suppose that the force employed is the pressure of a column of water of the diameter of the working-barrel; and this is over and above the force which is necessary for merely supporting the water at the height of the place of delivery. The motion of the water will be the same in both cases.

Let us, first of all, consider a sucking-pump. The motion here depends on the pressure of the air, and will be the same as if the pump were lying horizontally, and communicated with a reservoir, in which is a head of water sufficient to overcome all the obstructions to the motion, and produce a velocity of efflux such as we desire. And here it must be noted that there is a limit. No velocity of the piston can make the water rise in the suction-pipe with a greater velocity than what would be produced by the pressure of a column of water 33 feet high; that is, about 46 feet *per* second.

Let the velocity of the piston be V, and the area of the working-barrel be A. Then, if the water fills the barrel as fast as the piston is drawn up, the discharge during the rise of the piston, or the number of cubic feet of water *per* second, must be $= V \times A$. This is always supposed, and we have already ascertained the circumstances which ensure this to happen. If, therefore, the water arrived with perfect freedom to the piston, the force necessary for giving it this velocity, or for discharging the quantity $V \times A$ in a second, would be equal to the weight of the pillar of water whose height is $\frac{V^2}{2g}$, and base A.

It does not appear at first sight that the force necessary for producing this discharge has any thing to do with the obstructions to the ascent of the water into the pump, because this is produced by the pressure of the atmosphere, and it is the action of this pressure which is measured by the head of water necessary for producing the internal motion in the pump. But we must always recollect that the piston, before bringing up any water, and supporting it at a certain height, was pressed on both sides by the atmosphere. While the air supports the column below the piston, all the pressure expended in this support is abstracted from its pressure on the under part of the piston, while its upper part still supports the whole pressure. The atmosphere continues to press on the under surface of the piston, through the intermedium of the water in the suction-pipe, with the difference of these two forces.—Now, while the piston is drawn up with the velocity V, more of the atmospheric pressure must be expended in causing the water to follow the piston; and it is only with the remainder of its whole pressure that it continues to press on the under surface of the piston. Therefore, in order that the piston may be raised with the velocity V, a force must be applied to it, over and above the force necessary for merely supporting the column of water, equal to that part of the atmospheric pressure thus employed; that is, equal to the weight of the head of water necessary for forcing the water up through the suction-pipe, and producing the velocity V in the working-barrel.

Therefore let B be the area of the mouth of the suction-pipe, and C the area of the fixed valve, and let the suction-pipe be of equal diameter with the working-barrel. The head necessary for producing the velocity V on the working-barrel is $\frac{V^2}{2g}\left(\frac{A^2}{B^2}+\frac{A^2}{C^2}-1\right)$. If d express the density of water; that is, if d be the number of pounds in a cubic foot of water, then $d\,A\,\frac{V^2}{2g}$ will express the weight of a column whose base is A, and height $\frac{V^2}{2g}$, *all* being reckoned in feet. Therefore the force which must be applied, when estimated in pounds, will be $p=\frac{d\,AV^2}{2g}\left(\frac{A^2}{B^2}+\frac{A^2}{C^2}-1\right)$.

The first general observation to be made on what has been said is, that the power which must be employed to produce the necessary motion, in opposition to *all* the obstacles, is in the proportion of the square of the velocity which we would produce, or the square of the quantity of water we would discharge.

We have hitherto proceeded on the supposition, that there is no contraction of the jet in passing through these two orifices. This we know would be very far from the truth. We must therefore accommodate things to these circumstances, by diminishing B and C in the ratio of the contraction, and calling the diminished areas b and c; then we have $p=\frac{A\,dV^2}{2g}\left(\frac{A^2}{b^2}+\frac{A^2}{c^2}-1\right)$.

What this diminution may be, depends on the form of the parts. If the fixed valve, and the entry into the pump, are simply holes in thin plates, then $b=\frac{62}{100}\,B$ and $c=\frac{62}{100}\,C$. The entry is commonly widened or trumpet-shaped, which diminishes greatly the contraction: but there are other obstacles in the way, arising from the strainer usually put round it to keep out filth. The valve may have its contraction greatly diminished also by its box being made bell-shaped internally; nay, even giving it a cylindrical box, in the manner of Fig. 33. is better than no box at all, as in Fig. 5.; for such a cylindrical box will have the unaccountable effect of the short tube, and make $b = \frac{84}{100}$ B, instead of $\frac{62}{100}$ B. Thus we see that circumstances seemingly very trifling may produce great effects in the performance of a pump. We should have observed that the valve itself presents an obstacle which diminishes the motion, and requires an increase of power; and it would seem that in this respect the clack or butterfly-valve is preferable to the button-valve.

Example. Suppose the velocity of the piston to be 2 feet or 24 inches *per* second, and that the two contracted areas are each ⅕th of the area of the pump, which is not much less than what obtains in ordinary pumps. We have $\frac{V^2}{2g}\left(\frac{A^2}{b^2}+\frac{A^2}{c^2}-1\right) = \frac{576}{772}\,(25.+25-1) = 36{,}75$ inches, and the force which we must add to what will merely support the column is the weight of a pillar of water incumbent on the piston, and something more than three feet high. This would be a sensible portion of the whole force in raising water to small heights.

We have supposed the suction-pipe to be of the same diameter with the working-barrel; but it is usual to make it of smaller diameter, generally equal to the water-way of the fixed valve. This makes a considerable change in the force necessary to be applied to the piston. Let a be the area of the suction-pipe, the area of the entry being still B; and the equivalent entry without contraction being still b, we have the velocity at the entrance $= \frac{AV}{b}$, and the producing head of water $= \frac{A^2V^2}{2gb^2}$. After this the velocity is changed to $\frac{AV}{a}$ in the suction-pipe, with which the water arrives at the valve, where it is again changed to $\frac{AV}{c}$, and requires for this change a head of water equal to $\frac{A^2V^2}{2gc^2}$. But the velocity retained in the suction-pipe is equivalent to the effect of a head of water $\frac{A^2V^2}{2ga^2}$. Therefore the head necessary for producing such a current through the fixed valve, that the water may follow the piston with the velocity V, is $\frac{A\,V^2}{2gb^2}+\frac{A^2V^2}{2gc^2}-\frac{A^2V^2}{2ga^2}$, or $=\frac{V^2}{2g}\left(\frac{A^2}{b}+\frac{A^2}{c^2}-\frac{A^2}{a^2}\right)$. This is evidently less than before, because a is less than A, and therefore $\frac{A^2}{a^2}$ is greater than unity, which was the last term of the former formula. There is some advantage, therefore, derived from making the diameter of the suction-pipe less than that of the working-barrel: but this is only because the passage of the fixed valve is smaller, and the inspection of the formula plainly points out that the area of the suction-pipe should be equal to that of the fixed valve. When it is larger, the water must be accelerated in its passage through the valve; which is an useless expense of force, because this velocity is to be immediately reduced to V in the working-barrel. If the foregoing example be computed with a equal to one-fourth of A, we shall find the head H equal to 29 inches instead of 37.

But this advantage of a smaller suction-pipe is in all cases very moderate; and the pump is always inferior to one of uniform dimensions throughout, having the orifice at the fixed valve of the same area. And if these orifices are considerably diminished in any proportion, the head necessary for overcoming the obstacles, so that the required velocity V may still be produced in the working-barrel, is greatly increased. If we suppose the area a one-ninth of

A, which is frequently done in house-pumps, where the diameter of the suction-pipe does seldom exceed one-third of that of the working-barrel; and suppose every thing made in proportion to this, which is also usual, because the unskilled pump-makers study a symmetry which satisfies the eye; we shall find that the pump taken as an example will require a head of water = 13 feet and upwards. Besides, it must be observed, that the friction of the suction-pipe itself has not been taken into the account. This alone is greater, in most cases, than all the obstructions we have been speaking of; for if this pipe is three inches diameter, and that of the working-barrel is six, which is reckoned a liberal allowance for a suction-pipe, and if the fixed valve is 25 feet above the surface of the pit-water, the friction of this pipe will amount to one-third of the whole propelling force.

Thus we have enabled the reader to ascertain the force necessary for producing any required discharge of water from a pump of known dimensions; and the converse of this determination gives us the discharge which will be produced by any given force. For making $\frac{A^2}{b^2} + \frac{A^2}{c^2} - \frac{A^2}{a^2}$, (which is a known quantity resulting from the dimensions of the pump) $= M$, we have $H = \frac{V^2}{2g} M$, and $V^2 = \frac{2gH}{M}$, and $V = \sqrt{\frac{2gH}{M}}$. Now H is that part of the natural power which we have at command, which exceeds what is necessary for merely supporting the column of water. Thus, if we have a pump whose piston has an area of ¼th of a square foot, its diameter being 6¾ inches; and we have to raise the water 32 feet, and can apply a power of 525 pounds to the piston; we wish to know at what rate the piston will be moved, and the quantity of water discharged? Merely to support the column of water of this height and diameter, requires 500 pounds. Therefore the remaining power, which is to produce the motion, is 25 pounds. This is the weight of a column one foot four inches high, and H = 1,333 feet. Let us suppose the diameter of the suction-pipe ½ of that of the working-barrel, so that $\frac{A}{B} = 4$. We may suppose it executed in the best manner, having its lower extremity trumpet-shaped, formed by the revolution of the proper throchoid. The contraction at the entry may therefore be considered as nothing, and $\frac{A}{b} = 4$, and $\frac{A^2}{b^2} = 16$. We may also suppose the orifice of the fixed valve equal to the area of the suction-pipe, so that $\frac{A^2}{C^2}$ is also $= 16$, and there is no contraction here; and therefore $\frac{A^2}{c^2}$ is also 16. And, lastly, $\frac{A^2}{a^2}$ is also 16. Therefore $\frac{A^2}{b^2} + \frac{A^2}{c^2} - \frac{A^2}{a^2}$ or M, $= 16 + 16 - 16$, $= 16$. We have also $2\,g = 64$. Now $V = \sqrt{\frac{2gH}{M}}$. $= \sqrt{\frac{64 \times 1{,}333}{16}}$, $= 2{,}309$ feet, and the piston will move with the velocity of two feet four inches nearly. Its velocity will be less than this, on account both of the friction of the piston and the friction of the water in the suction-pipe. These two circumstances will probably reduce it to one foot eight inches; and it can hardly be less than this.

We have taken no notice of the friction of the water in the working-barrel, or in the space above the piston, because it is in all cases quite insignificant. The longest pipes employed in our deep mines do not require more than a few inches of head to overcome it.

But there is another circumstance which must not be omitted. This is the resistance given to the piston in its descent. The pistons of an engine for drawing water from deep mines must descend again by their own weight in order to repeat their stroke. This must require a preponderance on that end of the working-beam to which they are attached, and this must be overcome by the moving power during the effective stroke. It makes, therefore, part of the whole work to be done, and must be added to the weight of the column of water which must be raised.

This is very easily ascertained. Let the velocity of the piston in its descent be V, the area of the pump-barrel A, and the area of the piston-valve *a*. It is evident, that while the piston descends with the velocity V, the water which is displaced by the piston in a second is (A—*a*) V. This must pass through the hole of the piston, in order to occupy the space above, which is left by the piston. If there were no contraction, the water would go through with the velocity $\frac{A-a}{a}V$; but as there will always be some contraction, let the diminished area of the hole (to be discovered by experiment) be *b*; the velocity therefore will be $V\frac{A-a}{b}$. This requires for its production a head of water $\frac{V^2}{2g}\left(\frac{A-a}{b}\right)$. This is the height of a column of water whose base is not A but A—*a*. Calling the density of water *d*, we have for the weight of this column, and the force *p* is $d \times \overline{A-a} \times \left(\frac{A \quad a}{b}\right)^2 \times \frac{V^2}{2g}, = \frac{dV^2(A-a)^3}{2gb^2}$. This we see again is proportional to the square of the velocity of the piston in its descent, and has no relation to the height to which the water is raised.

If the piston has a button-valve, its surface is at least equal to *a*; and therefore the pressure is exerted on the water by the whole surface of the piston. In this case we shall have $p = \frac{dV^2A^3}{2gb^2}$ considerably greater than before. We cannot ascertain this value with great precision, because it is extremely difficult, if possible, to determine the resistance in so complicated a case. But the formula is exact, if b can be given exactly; and we know within very moderate limits what it may amount to. In a pump of the very best construction, with a button-valve, b cannot exceed one-half of A; and therefore $\frac{A^3}{b^2}$ cannot be less than 8. In this case, $\frac{V^2A^3}{2gb^2}$ will be $\frac{V^2}{8}$. In a good steam-engine pump V is about three feet per second, and $\frac{V^2}{8}$ is about 1⅛ feet, which is but a small matter.

We have hitherto been considering the sucking-pump alone: but the forcing-pump is of more importance, and apparently more difficult of investigation.—Here we have to overcome the obstructions in long pipes, with many bends, contractions, and other obstructions. But the consideration of what relates merely to the pump is abundantly simple. In most cases we have only to force the water into an air-vessel, in opposition to the elasticity of the air compressed in it, and to send it thither with a certain velocity, regulated by the quantity of water discharged in a given time. The elasticity of the air in the air-vessel propels it along the *Main*. We are not now speaking of the force necessary for counterbalancing this pressure of the air in the air-vessel, *which is equivalent to all the subsequent obstructions*, but only of the force necessary for propelling the water out of the pump with the proper velocity.

We have in a manner determined this already. The piston is solid, and the water which it forces has to pass through a valve in the lateral pipe, and then to move in the direction of the Main. The change of direction requires an addition of force to what is necessary for merely impelling the water through the valve. Its quantity is not easily determined by any theory, and it varies according to the abruptness of the turn. It appears from experiment, that when a pipe is bent to a right angle, without any curvature or rounding, the velocity is diminished about $\frac{1}{15}$. This would augment the head of water about $\frac{1}{7}$. This may be added to the contraction of the valve-hole. Let c be its natural area, and whatever is the contraction competent to its form increase it $\frac{1}{7}$, and call the contracted area c. Then this will require a head of water $= \frac{V^2A^2}{2gc^2}$. This must be added to the head $\frac{V^2}{2g}$, necessary for merely giving the velocity V to the water. Therefore the whole is $\frac{V}{2g}\left(\frac{A^2}{c^2}+1\right)$; and the power p necessary for this purpose is $\frac{dAV^2}{2g}\left(\frac{A^2}{c^2}+1\right)$.

It cannot escape the observation of the reader, that in all these formulæ, expressing the height of the column of water which would produce the velocity V in the working-barrel of the pump, the quantity which multiplies the constant factor $\frac{dAV^2}{2g}$ depends on the contracted passages which are in different parts of the pump, and increases in the duplicate proportion of the sum of those contractions. It is therefore of the utmost consequence to avoid all such, and to make the Main which leads from the forcing-pump equal to the working-barrel. If it be only of half the diameter, it has but one-fourth of the area, the velocity in the Main is four times greater than that of the piston, and the force necessary for discharging the same quantity of water is 16 times greater.

It is not, however, possible to avoid these contractions altogether, without making the main pipe wider than the barrel. For if only so wide, with an entry of the same size, the valve makes a considerable obstruction. Unskilful engineers endeavour to obviate this by making an enlargement in that part of the Main which contains the valve. This is seen in Fig. 14. at the valve L. If this be not done with great judgment, it will increase the obstructions. For if this enlargement is full of water, the water must move in the direction of its axis with a diminished velocity; and when it comes into the main, it must again be accelerated. In short, any abrupt enlargement which is to be afterwards contracted, does as much harm as a contraction, unless it be so short that the water in the axis keeps its velocity till it reaches the contraction. Nothing would do more service to an artist, who is not well founded in the theory of hydrodynamics, than to make a few simple and cheap experiments with a vessel like that of Fig. 37. Let the horizontal pipe be about three inches diameter, and made in joints which can be added to each other. Let the joints be about six inches long, and the holes from one-fourth to a whole inch in diameter. Fill the vessel with water, and observe the time of its sinking three or four inches. Each joint should have a small hole in its upper side to let out the air; and when the water runs out by it, let it be stopped by a peg. He will see that the larger the pipe is in proportion to the orifices made in the partitions, the efflux is *more diminished*. We believe that no person would suspect this who has not considered the subject minutely.

All angular enlargements, all boxes, into which the pipes from different working-barrels, unite their water before it goes into a Main, must therefore be avoided by an artist who would execute a good machine; and the different contractions which are unavoidable at the seats of valves and the perforations of pistons, &c. should be diminished by giving the parts a trumpet-shape.

In the air-vessels represented in Fig. 13. this is of very great consequence. The throat O, through which the water is forced by the expansion of the confined air, should always be formed in this manner. For it is this which produces the motion during the returning part of the stroke in the pump constructed like Fig. 13. No 1. and during the whole stroke in No 2. Neglecting this seemingly trifling circumstance will diminish the performance at least one-fifth. The construction of No 1. is the best, for it is hardly possible to make the passage of the other so free from the effects of contraction. The motion of the water during the returning stroke is very much contorted.

There is one circumstance that we have not taken any notice of, viz. the gradual acceleration of the motion of water in pumps. When a force is applied to the piston, it does not in an instant communicate all the velocity which it acquires. It acts as gravity acts on heavy bodies; and if the resistances remained the same, it would produce, like gravity, an uniformly accelerated motion. But we have seen that the resistances (which are always measured by the force which just overcomes them) increase as the square of the velocity increases. They therefore quickly balance the action of the moving power, and the motion becomes uniform, in a time so short that we commit no error of any consequence by supposing it uniform from the beginning. It would have prodigiously embarrassed our investigations to have introduced this circumstance; and it is a matter of mere speculative curiosity: for most of our moving powers are unequal in their exertions, and these exertions are regulated by other laws. The pressure on a piston moved by a crank is as variable as its velocity, and in most cases is nearly in the inverse proportion of its velocity, as any mechanician will readily discover. The only case in which we could consider this matter with any degree of comprehensibility is that of a steam-engine, or of a piston which forces by means of a weight lying on it. In both, the velocity becomes uniform in a very small fraction of a second.

We have been very minute on this subject; for although it is the only view of a pump which is of any importance, it is hardly ever understood even by professed engineers. And this is not peculiar to hydraulics, but is seen in all the branches of practical mechanics. The elementary knowledge to be met with in such books as are generally perused by them, goes no farther than to state the forces which are in equilibrio by the intervention of a machine, or the proportion of the parts of a machine which will set two known forces in equilibrio. But when this equilibrium is destroyed by the superiority of one of the forces, the machine must move; and the only interesting question is, *what will be the motion?* Till this is answered with some precision, we have learned nothing of any importance. Few engineers are able to answer this question even in the simplest cases; and they cannot, from any confident science, say what will be the performance of an untried machine. They guess at it with a success proportioned to the multiplicity of their experience and their own sagacity. Yet this part of mechanics is as susceptible of accurate computation as the cases of equilibrium. We therefore thought it our duty to point out the manner of proceeding so circumstantially, that every step should be plain and easy, and that conviction should always accompany our progress. This we think it has been in our power to do, by the very simple method of substituting a column of water acting by its weight in lieu of any natural power which we may chance to employ.

To such as wish to prosecute the study of this important part of hydraulics in its most abstruse parts, we recommend the perusal of the dissertations of Pitot and Bossut, in the Memoirs of the Academy of Paris; also the dissertations of the Chevalier de la Borda, 1766 and 1767; also the *Hydraulique* of the Chevalier de Buat.

END OF VOLUME SECOND.

Oliver & Boyd, Printers, Edinburgh.

STEAM ENGINE.

VOL. II. PLATE. 1.

Fig 1.

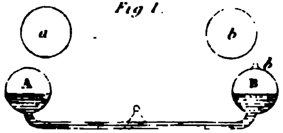
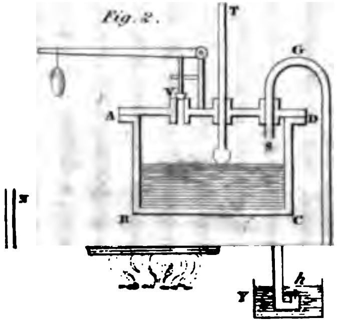


Fig. 2.



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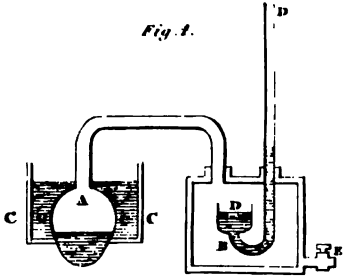

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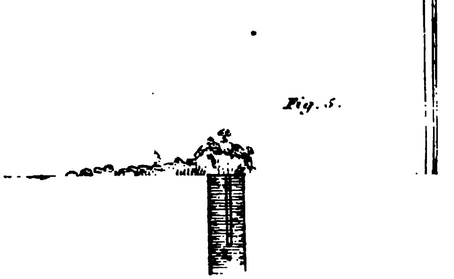

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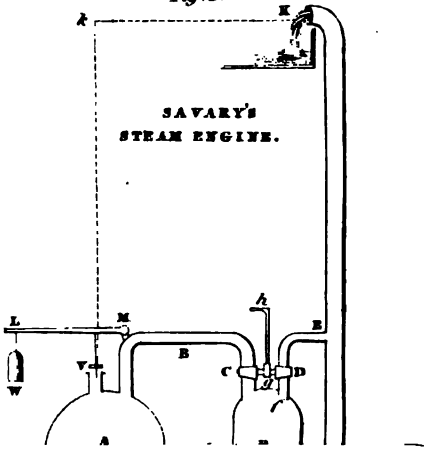
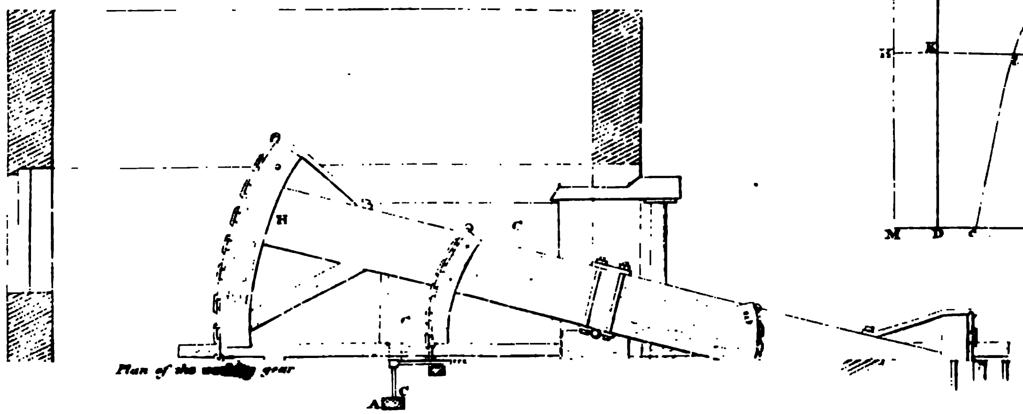


Engraved by W. & D. Lizars Edinburgh.

Fig. 8. No. 1.

BEIGHTON'S STEAM ENGINE.



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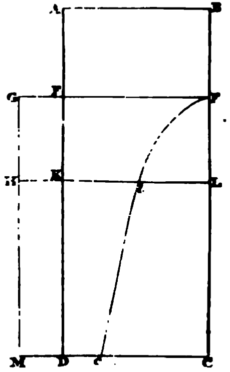


Wm. Creighton del.

Engd. by W. & D. Lizars Edinr.

STEAM ENGINE.

VOL. II. PLATE. III.

Mr. WATT's DOUBLE STEAM ENGINE from his SPECIFICATION of 1782.

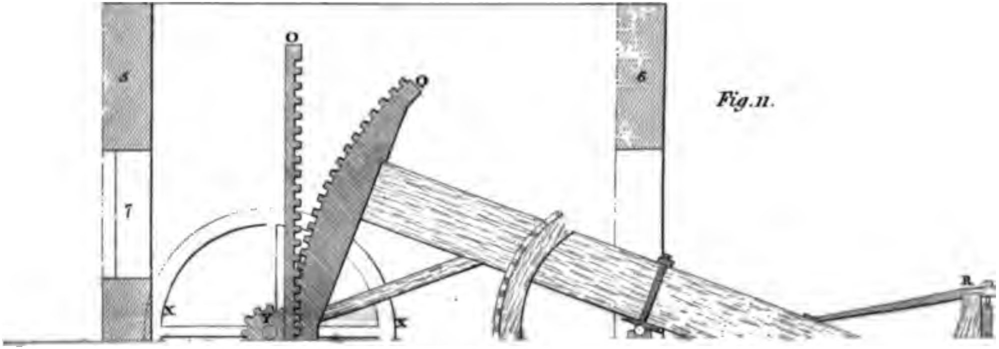
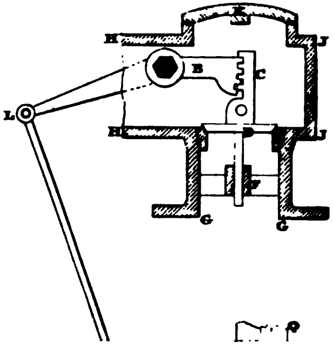


Fig. 11.

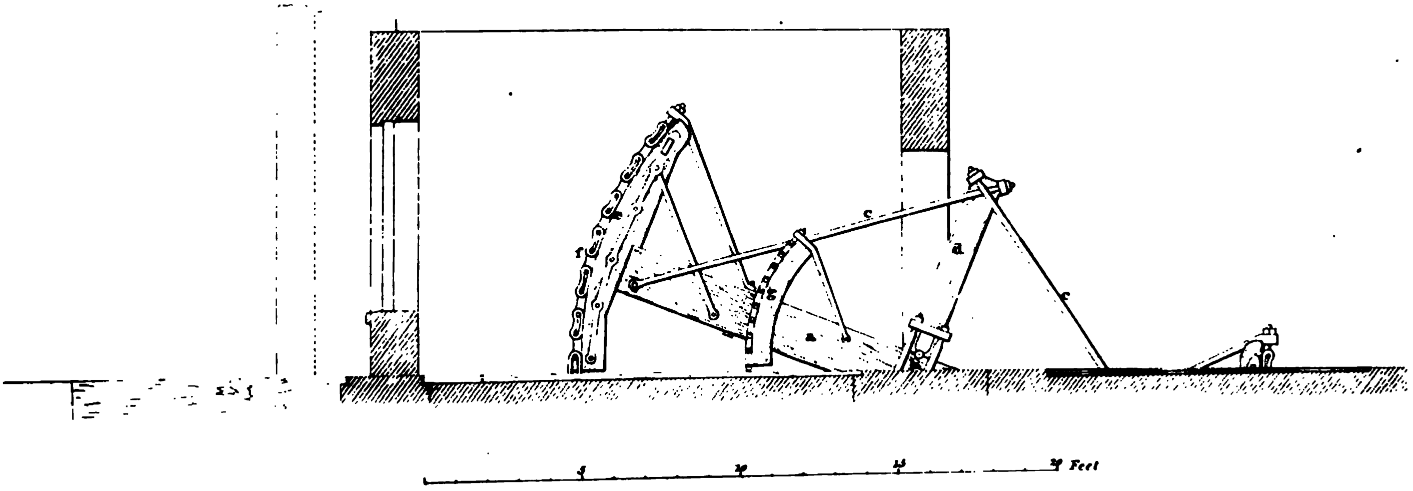
Fig. 13.



STEAM ENGINE.

VOL. II. PLATE IV.

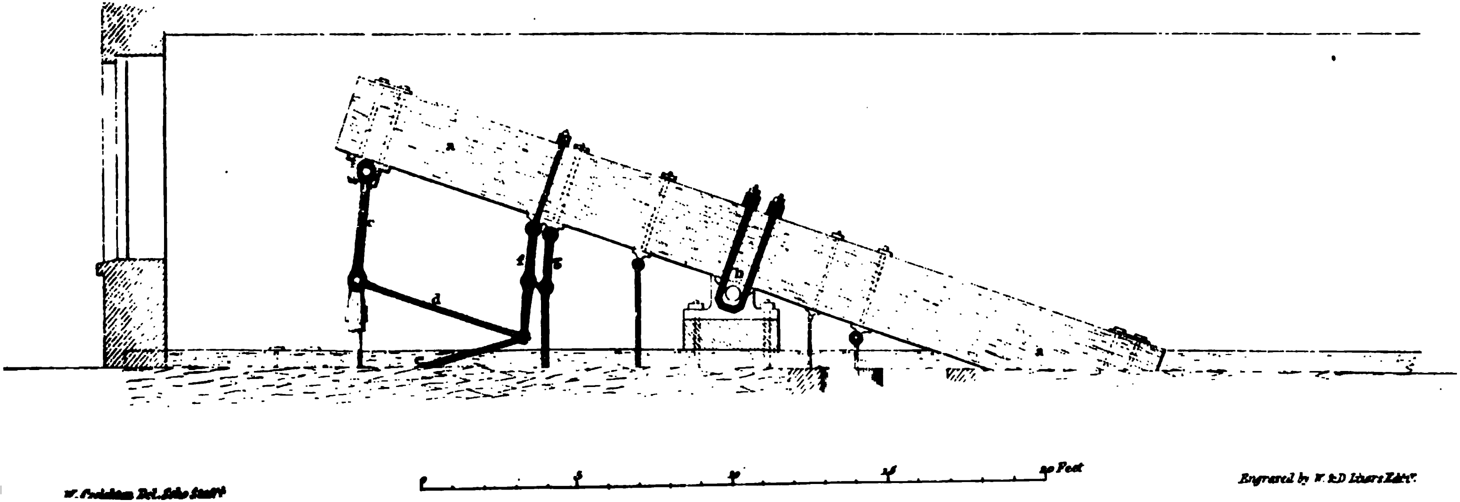
Mr. Watt's Single Reciprocating Engine as Constructed 1788.



STEAM ENGINE.

VOL. II. PLATE. V.

SIDE VIEW or SECTION, of ONE of the ALBION MILL STEAM ENGINES. 1787.



W. Creighton Del. Soho Staffs

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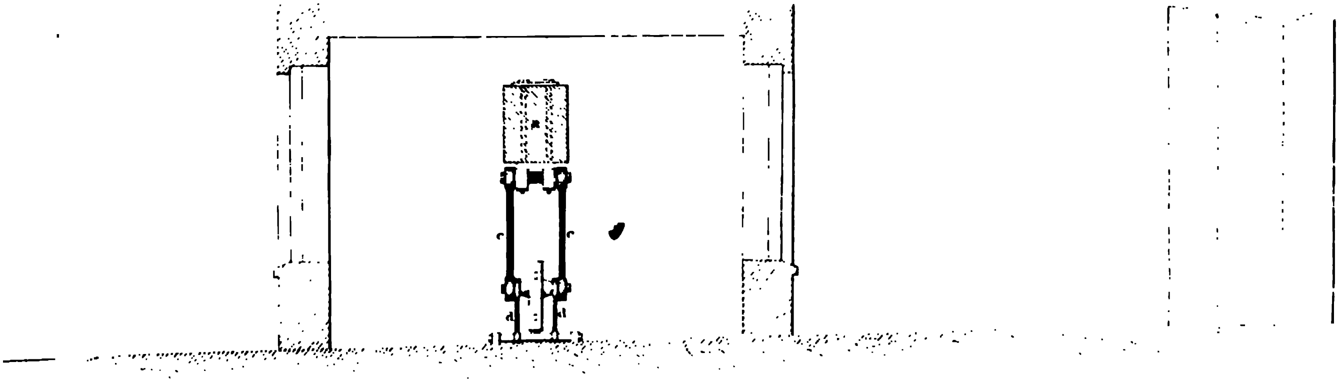
STEAM ENGINE. VOL. II. PLATE. VI.

CROSS SECTION of the ALBION MILL ENGINE and BOILERS.

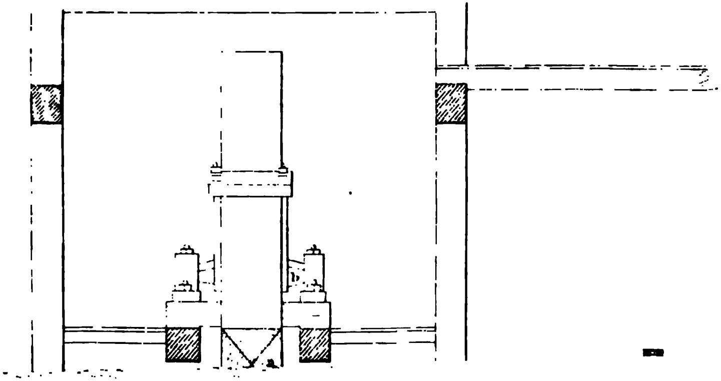


[illegible]

Engraved by [illegible]

STEAM ENGINE.

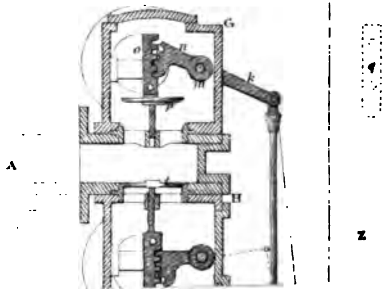
EDGE VIEW of the ROTATIVE MOTION of ONE of the ALBION MILL ENGINES.



SECTION

Manner of Opening and Shutting the Nozle

Fig. 2.



W. Creighton Del. Soho Staffd

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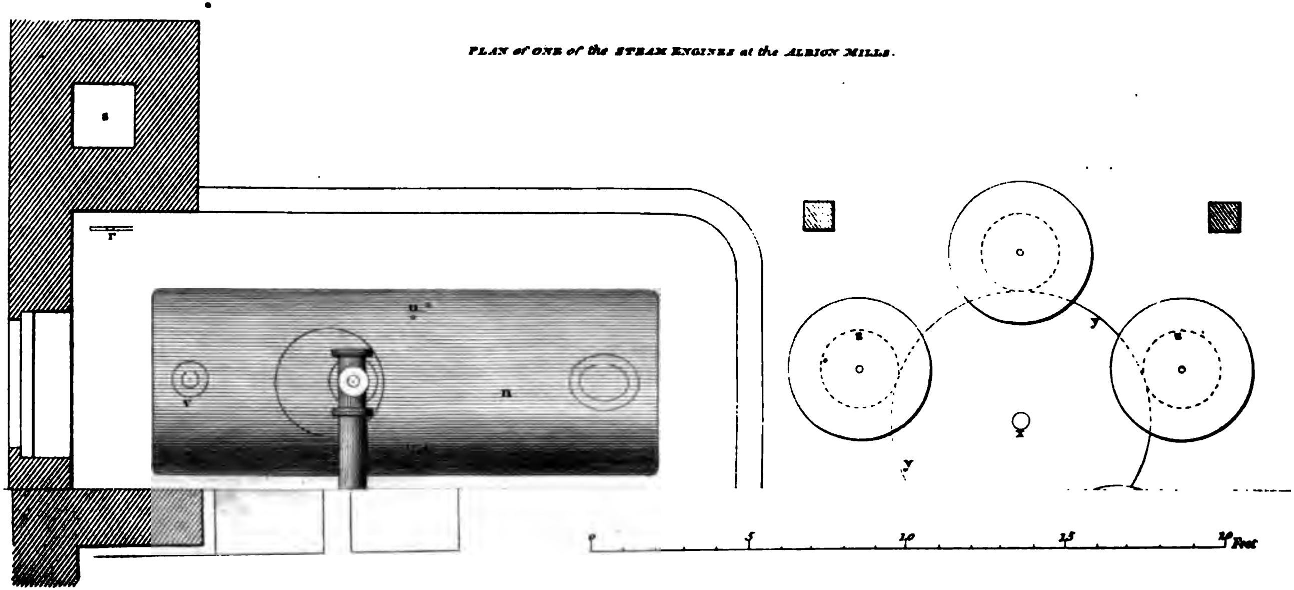
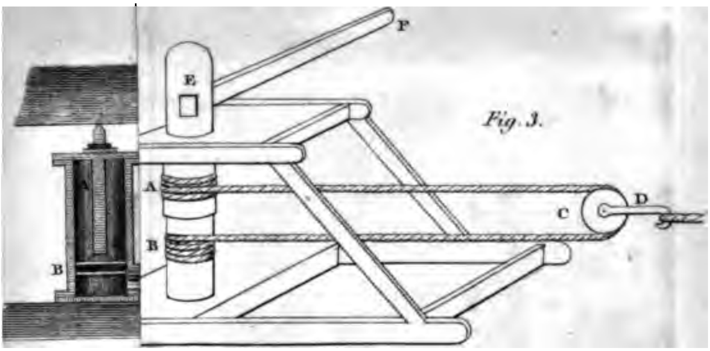
STEAM ENGINE.

VOL. II. PLATE.

PLAN of ONE of the STEAM ENGINES at the ALBION MILLS.



MACHINERY. *VOL. II. PLATE VIII. No. II.*



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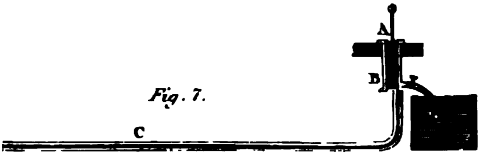


Fig. 7.

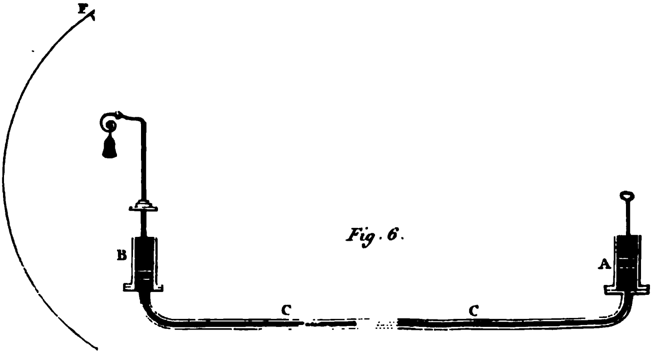
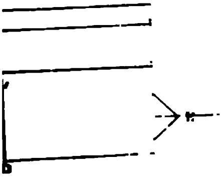


Fig. 6.

Eng.d by W.H.Lizars

NCE OF FLUIDS VOL. II. PLATE IX



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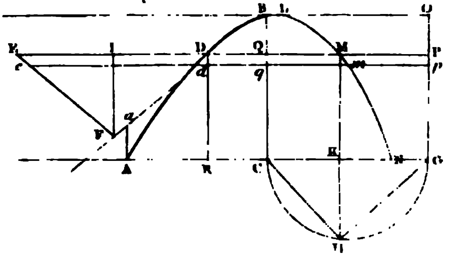

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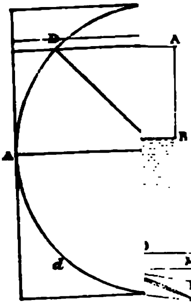

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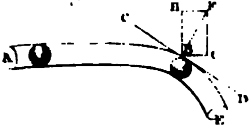

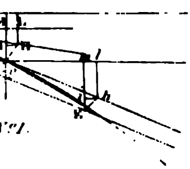

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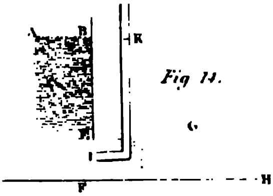

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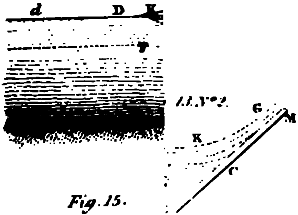

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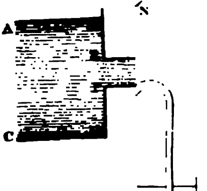
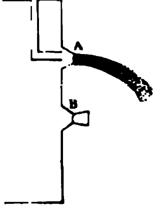
Fig. 15.





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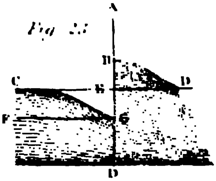

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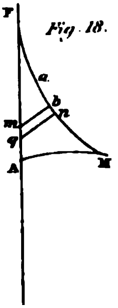

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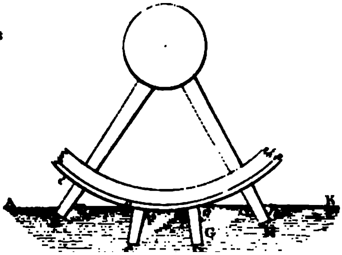

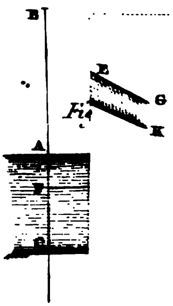
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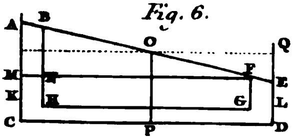

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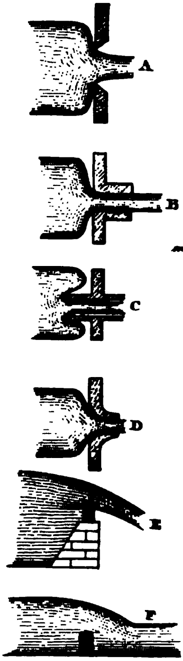

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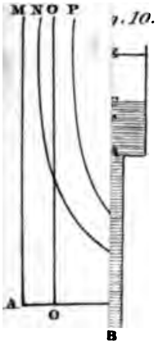

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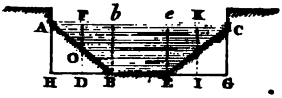

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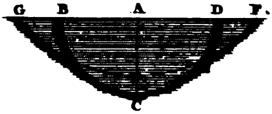


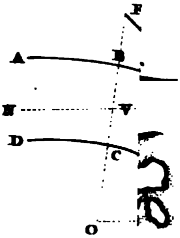
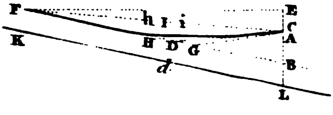


Fig. 16

Fig. 22.



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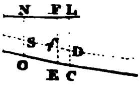

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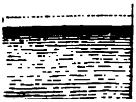

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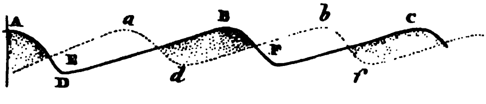
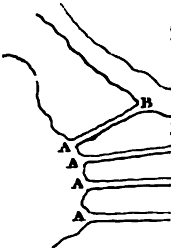


WATERWORKS VOL.II. PLATE XI.



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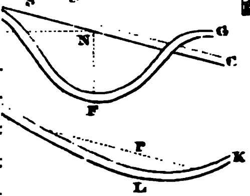


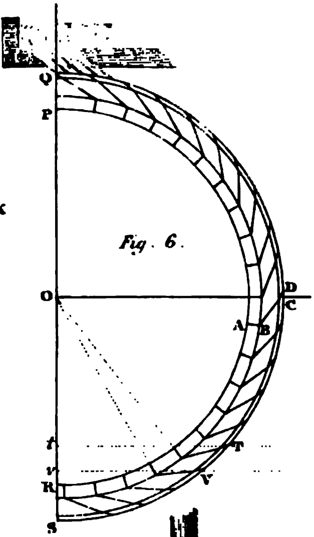
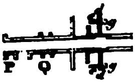


Fig. 6.

Fig. 20.



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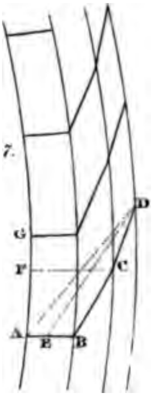

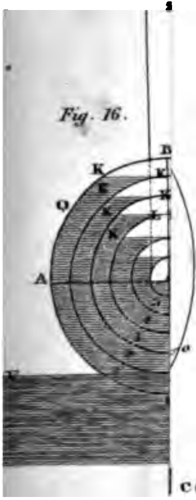
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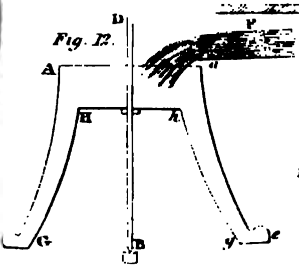
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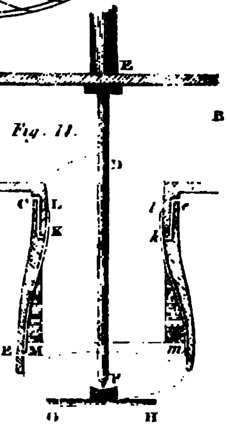
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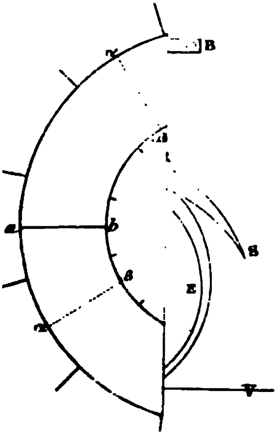


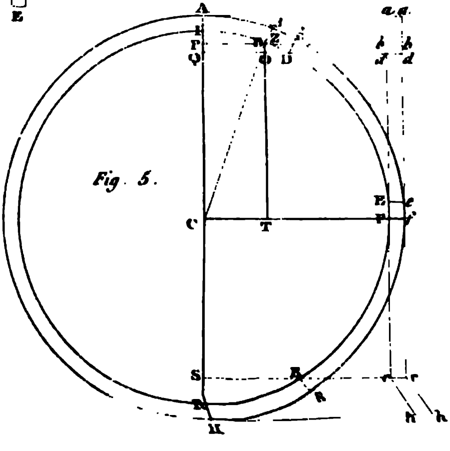


Fig. 5.

PUMP. VOL. II. PLATE XII.

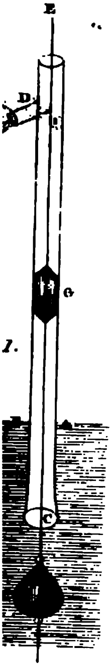
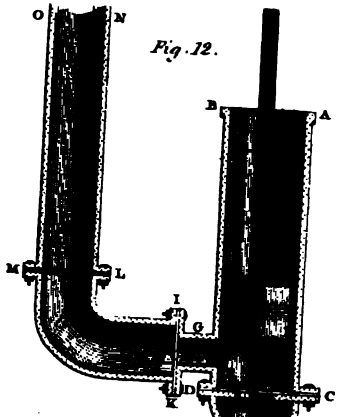


Fig. 11.

Fig. 12.



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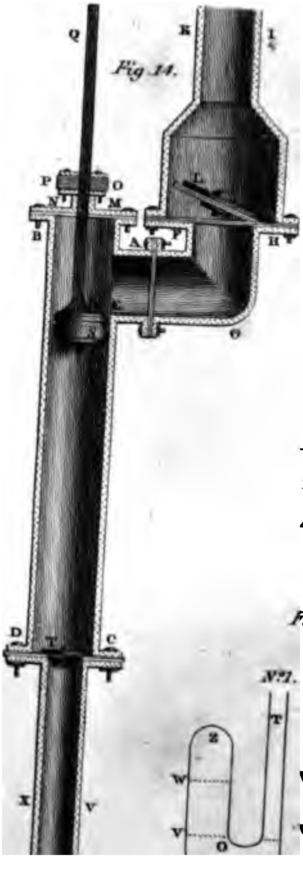


ig. 15.

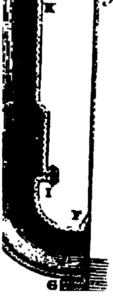
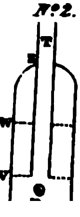


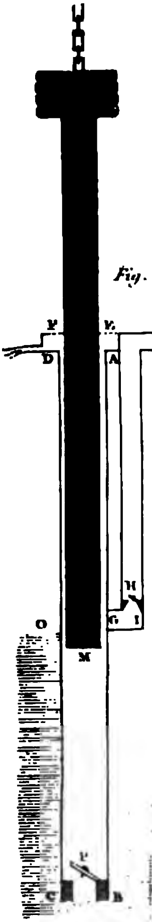
Fig. 13.

N°1.

N°2.



PUMP VOL. II. PLATE XIII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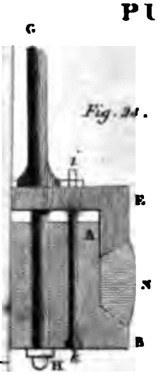
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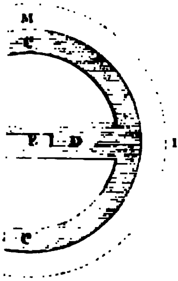
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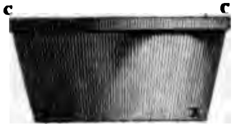
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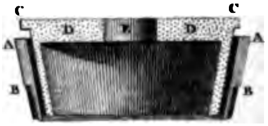
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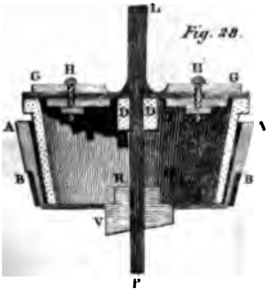
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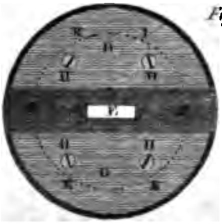
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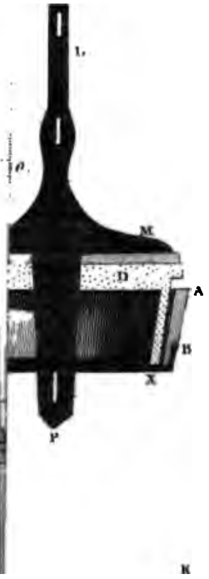
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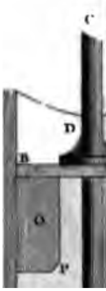


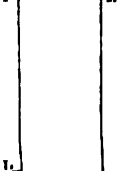
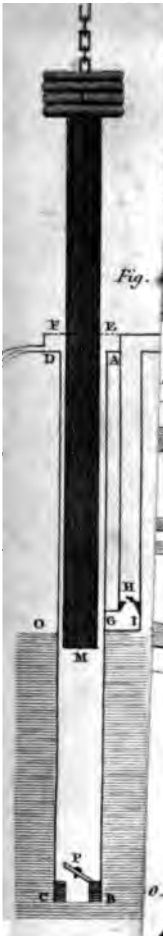


Fig. 37.

Fig.



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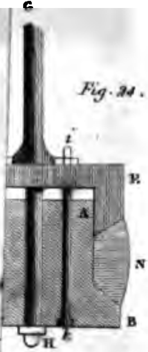

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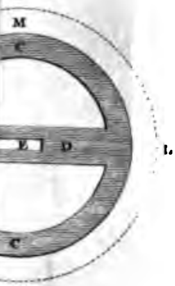


Fig. 25.

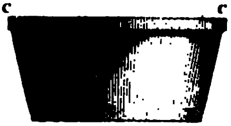


Fig. 26.

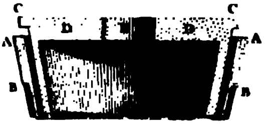


Fig. 28.

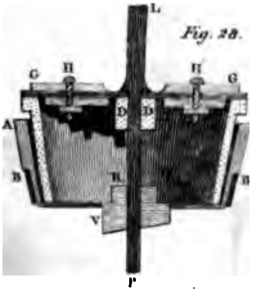


Fig. 29.

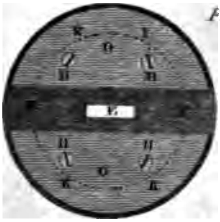
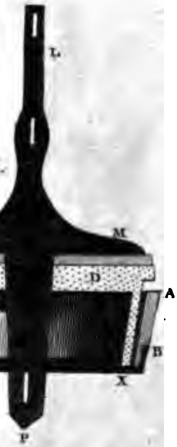


Fig.



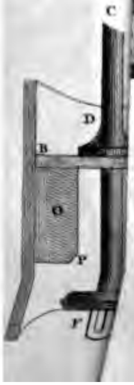
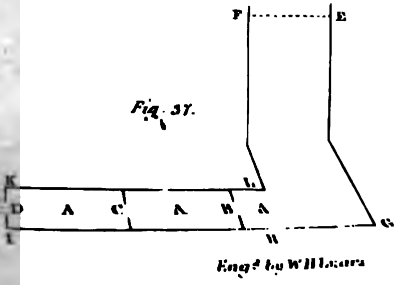


Fig. 37.



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